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VOLUME 7

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BIOGRAPHICAL MEMOIRS OF FELLOWS OF THE ROYAL SOCIETY

1961
Volume 7

Department of Zoology Library,
Imperial College, Beit Building,
Prince Consort Road, London, S.W.7.

Published by The Royal Society
Burlington House
Piccadilly
London, W.1

Published November 1961

PRINTED BY
HEADLEY BROTHERS LTD
LONDON, AND ASHFORD, KENT

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A photograph of the subject is printed with each notice

W. Lawrence Balls.

WILLIAM LAWRENCE BALLS

1882-1960

WILLIAM LAWRENCE BALLS, the only son of William and Emma Mary Lawrence, was born on 3 September 1882 at Garboldisham Mill in south Norfolk. His many generations of East Anglian forebears, some from the land —farmers, and millers, and others of the water—craftsmen, carpenters, and boatbuilders, contained an added French strain on his mother's side and a Dutch one—Bols, Bull, on his father's.

His father was pushed into teaching by his father, at St John's School, Lowestoft, and he married just after becoming Headmaster of Garboldisham School. After a year at Beccles he became Headmaster of the National School at Diss in Norfolk, and there spent the rest of his working life, retiring to his birthplace at Oulton Broad and dying at eighty-seven in 1939. His mother was Headmistress with him at Diss, but died in her thirties of septicaemia.

His father was a craftsman; he built his own bone shaker bicycle, a billiard table and a motor boat. His mother regarded every living thing as a friend, from cat to toad. Her mother had been left a young widow with three children, but ran her own windmill and farm for decades, dying at ninety-five in 1922.

As Balls himself said, his mother made a biologist of him and his father a craftsman. Marking out with square and compasses in his father's workshop introduced him to geometry. Geometrical drawing led to Euclid and graph technique, whereby he was able to survive the worst of the limitations imposed on him by an unimaginative, algebra-grinding master at his school.

Although Balls described himself as a craftsman, he never regarded himself as a real one. He was content with knowing what tools to use, and how to use them to make something which would last as long as it was wanted to serve some need. His interest in workshops lay more in the result than in the making. This early training in craftsmanship was invaluable to him in later years, when he was to concern himself with textile technology, and when it was necessary to improvise or invent apparatus.

Balls was a competent artist, doubtless deriving this trait from his father who had at some time qualified as an art teacher. One of his father's prizes at the St John's Art evening school was a biography of Humphry Davy by Henry Taylor. This book was one of the factors which determined the course of Balls's life.

As a biography it was not especially veracious, but it managed to blend human interest with the charm of things inanimate, and with the experiences of the ploddings and stumblings which led to the safety lamp. To a small boy this book had great appeal.

Still in the pre-Grammar School period, Balls attended the classes of the Science and Art Department of South Kensington, held in the magistrates' room at Diss Corporation Hall. His father's teaching was there supplemented by that of a tall lecturer with a shock head of dark curls. This was T. B. Wood of the School of Agriculture at Cambridge. Balls's interest in entomology was aroused by the local curate, who led him into insect breeding, by means of which he was later able to talk shop effectively with his entomological colleagues in Egypt. Botany, apart from some participation in gardening and farming in the holidays, had not yet come his way. His father offered him a bicycle if he got into the first class of the yearly diocesan examination in religious knowledge for all the church schools in the county of Norfolk. This bribe succeeded beyond all expectation, for he came out second on the list of some two thousand children, and surpassed this feat the following year by coming out first. The immediate advantage was the bicycle, but he made the discovery that examinations would surrender to a suitable technique of concentration while studying, coupled with the presentation of answers in such orderly form as would ease the fatigue and boredom of examiners.

His interest in music was never deep seated. He had a good voice and was given the choice between the Norwich Cathedral choir and that of the Middle School. He went through the stages of church choir, choral society, woodwind, and piano, sight reading for accompanying singers in college rooms, interest in Italian opera in Cairo. He even acted occasionally as reserve musical critic for the *Egyptian Gazette*. In later life his interest faded.

Literary interests began early. He was found able to read when still an infant, and nobody knew how he had effected the synthesis of sounds and letters. His memory and imagination were always visual and this perhaps accounted for the ability to read at 'reviewer's speed' even in childhood. Reprimands for turning the pages too quickly ceased when it was found that he was spotting misprints in the process. As an undergraduate, this ability enabled him to cover a wide range of English literature as a side issue to his science studies.

With a wide range of interests embracing literature, art, music, farming and craftsmanship, he went to King Edward Middle School at Norwich. This school was a modernized offshoot of the Grammar School which Nelson had attended. It was housed by much adaptation in the cloisters of the Benedictine monastery behind St Andrew's Hall, a setting which made history a reality. The yard-stick of achievement was the Cambridge Local examination. The teaching was not imaginative but as solid as the masonry of the old monastery, which defied the picks of workmen trying to build a new technical school over the playground wall. Formal science teaching was just chemistry of the most factual kind, but there was a usefully large laboratory which later became a more or less personal study when he was captain of the school in 1898-1899, and where he nearly lost both eyes through ignorant handling of sulphuric acid in a test tube generator. Unauthorized visitors were deterred by a Ruhmkorff coil wired to the laboratory door handle.

Travelling twenty miles to Norwich by train every day was something of a handicap for sport, but he played soccer, took no interest in cricket and was a potential star at cycle racing. This pastime came to an end under family pressure when he broke a collar bone in an accident caused by touching the wheel of the next ahead.

Cambridge was made possible financially, but only just, by the award of a scholarship from the Norfolk County Council. It also awarded him the 'Richard England Prize' for which he obtained a microscope of his own. He was young for Cambridge, but fitted in two terms as an unattached student with T. B. Wood in the Agricultural Department, joining St John's as a by-termist in the May term of 1900. There he ran into a shortage of Little-Go Greek and had to spend most of the following term cramming to retrieve a previous failure. Fortunately he knew the set Gospel by heart, and so could translate nicely as long as the beginning and end of the set passage could be identified. He was awarded a College Exhibition, which with his County Scholarship made him self-supporting for the next two years. Meanwhile the death of his mother had deprived her of seeing the result of her devotion.

Part One of the Tripos in those days was taken in four subjects. He did this in his second year, getting a First and coming top of the list in geology. His four subjects were chemistry, geology, botany and human physiology. Though stimulated by McKenny, Hughes and Marr, and keenly interested in Harker's petrology and Wood's palaeontology, he had regretfully to discard geology as a means of making a living and therefore took up botany for Part Two of the Tripos. His intention was to use this in agriculture for which purpose he spent his third year in getting the Agricultural Diploma, then returning to the new Botany School.

At that time botany in Cambridge was full of interest, with Bateson at Grantchester to be assisted in his early researches on genetics, and also Biffen who was in the middle of his work on the genetics of rust resistance in wheat. Two long vacations were spent on the University farm at Impington. Marshall Ward, the Professor, had spent years trying to correlate rust resistance with morphology in *Bromus*, and Balls was the sole spectator when Biffen first showed him the green and yellow wheat plots demonstrating Mendelian segregation of immunity from rust in his wheats. Balls never 'clicked' with Marshall Ward, for whom he felt rather sorry. He was, he says, too opinionated about what a botanist should do and be. He disliked systematy but acquired cytological technique with the help of Barker. In the oral of Part Two he got a grievous roasting and was surprised when he was placed in the first class along with Agar, Drummond, and Miss Wheldale.

Still evidently holding a dim view of his usefulness, Marshall Ward ran him without conviction for an uninteresting job in British Guiana, which he fortunately failed to get. Quite on his own he answered an advertisement in *Nature* and picked up the new post of 'Cryptogamic Botanist' to the Khedivial Agricultural Society in Cairo. He had done a little mycological research, getting a cross infection of rust from wheat to couch grass, after persuading germ tubes to enter artificial stomata, and he knew through F. F. Blackman's teaching of plant physiology that such studies called for the invention and construction of gadgets. This he was quite prepared to do. The cryptogamic botanist was wanted to work on wilt disease in cotton, but by the time he reached Cairo in November 1904 this was found to be unnecessary. The scare about wilt disease was a false alarm, due to a mistake in diagnosis, so that he was left with a free hand in a brand-new job, to see how far Mendel's Law could be used in cotton breeding, and to combine this with physiology, unlimited except by staff and funds.

Egypt 1904-1914

He was destined to work in Egypt for the next decade. For the first six years he was in the service of the Khedivial Agricultural Society, and had freedom to conduct researches as he chose. For this exceptional treatment he acknowledged his indebtedness to H.H. Prince Hussein Pasha Kamel, President of the Society and to Mr G. P. Foaden, the Director. For the last four years his position was less congenial as he was transferred as Botanist to the newly founded Department of Agriculture. It saddled him with a new chief, 'new to Egypt and new to science', with whom his relations were a prolonged worry. The sight of an official-looking envelope sent shivers down his back for a year or more after leaving Egypt altogether.

Facilities in the first few years were restricted. Foaden gave him an acre of land on the farm at Giza, some three miles from the laboratory, and a tent to serve as a field laboratory. He acquired a splendid black gardener, ex-driver of a stone cart who taught himself to read and write both English and Arabic. Francis Holton came from the School of Agriculture as assistant in 1910. With these facilities, described to me as roughly one acre, one tent and one lab. boy, he was able to study his first cotton crop in 1905, using single boll cultures obtained from the Agricultural Show of the previous year. In this first period of his Egyptian service he studied nine successive cotton crops in great detail. The work began as genetics, but necessarily extended into physiology, and towards the end of this period it had encroached upon and was revivifying textile technology. Bateson, with his rigorous genetical techniques and F. F. Blackman, with his 'law of the limiting factor', contributed about equally to his outlook. He felt free to use any technique from any discipline whatever for the furtherance of the ideas which came to him so rapidly and so fruitfully. This decade was extraordinarily productive and in it he published 45 papers and a book, *The cotton plant in Egypt*, which has taken its place as a biological classic. In this book published in 1912, he summarized and added to his studies in genetics and physiology.

His general aim was to find out as much as possible about cotton, on the assumption that any facts about a useful plant are bound to be useful at some time. The studies dealt with cotton in the dual aspects of heredity and environment in its widest sense, crop physiology and ecology. Later it included the origin, development, and properties, both physical and industrial of the cotton hairs which were the raw material of commerce.

His first cotton crop provided him with essential information on segregation, dominance and natural crossing, the seed characters having been recorded in the parental single boll samples. It was clear from the results that 5 to 10 per cent of the seeds were not self-fertilized. The avoidance and control of this source of error became fundamental to all subsequent work, whether scientific or economic. He was led to an understanding of why there had been in Egypt a constant succession of new varieties, each of which degenerated and became lost after a few years of cultivation.

He saw first that these varieties, being single plant selections open to natural crossing even in the year of their selection, were only approximately pure at the beginning and would inevitably degenerate either through natural crossing, mechanical mixture or both these causes. The remedy was continued self-fertilization to the stage of practical homozygosity, then selection on the basis of both agricultural and industrial properties and then conservation of purity by the most refined techniques of isolation possible —techniques almost bacteriological in their thoroughness. It is understandable that he believed the pure line concept of Johannsen to be as important, possibly even more important, than the principles of Mendel.

In subsequent generations from these small cultures he attempted to record every observable character of every individual plant. His manuscript notes say that 'this was with the idea of demonstrating that characters might be inherited in groups because they were carried on one and the same chromosome. This was at a time when the chromosome nature of inheritance was quite a heresy.' There is, however, no inkling in any of his published works that this idea was at the back of his mind. If it was, it is an example of scientific intuition of the most penetrating kind, not unusual for him to display. It could explain why he preliminarily carried out researches into the cytology and chromosome number of cotton. Before 1905 there was only one solitary pointer to the role of chromosomes in heredity formulated in modern terms. Sutton in 1903 ('The chromosomes in heredity', *Biol. Bull.* **4**, 231) had said that 'all the allelomorphs represented by any one chromosome must be inherited together'. It was not till 1910 that even Morgan was convinced of the validity of the chromosome theory of heredity and Bateson held out against it till 1921.

The contributions of Balls to our knowledge of cotton genetics were not weighty, and seldom more than merely suggestive. They were, however, enormously stimulating to workers in other parts of the world, who became convinced that cotton would yield to Mendelian analysis, it being held by some that it was abnormal in this respect. His lack of pronounced success was due to a variety of reasons. The technical difficulties involved in getting enough selfed material were great, and he added to them by working with a species hybrid, Upland (*Gossypium hirsutum*) × Egyptian (*Gossypium barbadense*) and by studying quantitative characters which did not yield to formal analysis because they showed blending in the second generation. He was seldom able to observe more than a limited number of families in the third generation, with small numbers of plants in each, and he was obliged to use open pollinated seed. The very difficulties, however, led him into interesting and sometimes profitable bypaths. The difficulty of securing self-fertilized seed led to researches on natural crossing, for which he used the term vicinism. If he could not use self-fertilized seed he had to find methods of spotting 'rogues' in his cultures. He thought of ways of breeding cottons immune from natural crossing. At one time he thought he had solved the problem by breeding a plant with a short stigma surrounded entirely by anthers in such a way that foreign pollen could not get through. He got such a plant but it was still subject to natural crossing. He therefore embarked on a series of researches into the prepotency of self pollen as compared with pollen from another species and from the species hybrid F_1. Anticipating the maize work of Jones in Connecticut by several years he established that self pollen was prepotent over foreign pollen but that some gametes of an F_1 were prepotent even over self pollen. Accordingly he suggested the possibility, later unsuccessfully explored by the writer, of breeding a cotton immune from natural crossing. Recent work on uncrossable maize has brought his ideas, forgotten for half a century, sharply into focus.

The phenomenon of apparent blending inheritance he tackled with his customary originality. He observed that for quantitative characters the composition of an F_2 was commonly similar to a Gaussian curve of error. He concluded, however, that this similarity might be fictitious and 'due to the appression of several true modes which blend into one another by fluctuation. Part of the blending, moreover, is not fluctuation in the ordinary sense but rather an *autogenous fluctuation* provoked by correlation with other characters'. This could be taken account of by plotting separate curves for plants with similar characteristics, such as leaf or boll size or boll width. In a study of the inheritance of seed weight he made a number of such dissected curves from the form and modes of which he deduced that large seed was segregating from small seed in a simple 3:1 ratio, a single pair of allelomorphic characters being involved. The weakness of this theory lay in the fact that both parents were light seeded. One parent (Upland) bore the seeds in a large boll, while the other (Egyptian) carried them in a small one. On crossing, the Egyptian seed was placed inside a boll of double the cubic content, and an increase in size followed. The Egyptian seed was genetically large and dominant over the small Upland one, but could not manifest itself because it was in a small boll. This method of approach seems to be logical for a good many quantitative characters but has not been used to any extent. The tendency of Mendelian workers at that time to interpret their results in terms of pairs of simple allelomorphs was shared by Balls, and the inheritance of seed size and other quantitative characters with which he worked was certainly far more complicated than he envisaged. What is illuminating, however, is the way his mind worked when confronted with an apparently intractable problem.

He left formal genetics for good after seven years' work, with extensive records of growth behaviour and quantitative data destined to be useful in crop physiology. He also had a few approximately pure lines which ultimately revolutionized current ideas about the role of the grader in assessing the value of the raw material, and paved the way towards a really scientific supply of seed for Egypt. The time was approaching when botany and textile technology would join forces.

Balls once remarked that his two main interests were growth and gadgets. In his work on cotton physiology both these were highly involved. At their first meeting, Foaden had told him that the yield of cotton seemed to be falling from causes unknown. He made a note of this as worthy of investigation. One of his early papers dealt elegantly with the relationship of growth to temperature in the sore shin disease, which caused a troublesome damping off in the breeding plots. The results on temperature coefficients, optima determinations, and the staling of cultures led to the conception of 'depressant factors', and to that of thermotoxy. These conceptions were interwoven throughout all his work on crop physiology, which justified his giving much attention to gadgets, to autographic recorders for growth, for clouds at night, wind force, stomatal apertures, leaf temperatures, etc.

Another stimulus to crop physiology was provided by the 'water table hypothesis' put forward by Audebeau of the State Domains, with whom Balls exchanged ideas in Provençal French. Audebeau and Ferrars produced the heretical opinion, backed by ample data, that the improvements in the irrigation system were reducing the depth of soil available for the root system of cotton, and were thus reducing the crop. Much of Balls's thinking on crop physiology centred round this still unpopular theory. More than half a century later his last book, *Yields of a crop*, showed that Audebeau and Ferrars were right.

His plant development studies were concerned with methods for recording the building up of the cotton crop under field conditions. By counting daily the number of flowers on experimental plots it was possible to express flowering graphically as a curve of number of flowers per plant per day and since the maturing period of the boll was about seven weeks a similar curve of bolls per plant would give exact information on the magnitude of the crop and the time of its arrival. By estimating the difference between the two curves the extent of loss of crop by shedding and other factors could be determined. Behaviour of the crop was simplified by reducing the results to the average plant in the plot or pedigree selection. In some sense the work was an amplification of the technique of statistical treatment of agricultural plot experiments initiated by Wood and Stratton early in the century. It was, however, more informative since if two strains gave similar yields they might do this by different paths. Developmental curves could specify exactly how the crop was being built up in each case. The application of this powerful method to the important agricultural problems of sowing time and density of spacing was elaborated in two major papers under the title 'Analyses of agricultural yield'. As a result of this work counting of flowers and bolls became fashionable for a time throughout the cotton growing world. At the present time such curves are no longer used, partly because their interpretation demands more intelligence than is possessed by the usual cotton worker, and partly because the taking of the records requires non-existent cheap labour.

In his search for the causes of the decline of the Egyptian cotton crop Balls carried on extensive studies of root behaviour in connexion with water tables and the subterranean environment of the plant. His researches threw into prominence the 'immense importance of the root system whereof, in a limited sense the aerial portions are only the visible expression'. He put forward the view that a great deal of botanical research during the coming twenty years—he was writing in 1912—would be subterranean. For most crops this view is still not apprehended.

During the last three years of this decade in Egypt he was instructed to cease work on water tables and confine himself to plant breeding. He was now in a government department of applied science, in contrast to the free indulgence of long-term curiosity which the Khedivial Society had encouraged. Under orders to show whether he had any cottons of real practical value he picked out from his files a few more-or-less pure lines and began to propagate them on a commercial scale. Without waiting for another year to grow normal field crops of each in several localities, the lints from these propagation areas were submitted to a jury of commercial cotton graders without interpretation or explanation. They said that some were good and some were ordinary; none were miraculous. So Kitchener, then Agent-General, decided that his contract was not to be renewed. Opinions on the merits of his work were not unanimously unfavourable. While being seen off at Cairo Station by his former colleagues in the Society he had the embarrassing experience of being wept over and kissed by Charles Audebeau, but the stark truth was that he was thrown out, with his ten years' work apparently wasted and a bad official reputation.

One month before leaving Egypt he was visited by John McConnell of the Fine Spinners' and Doublers' Association of Manchester. He and Balls took to one another immediately. They discovered an identity of interest and ignorance. Balls, during his leaves of absence, had periodically visited Lancashire and had tried to find out from spinners what sort of cottons he should breed for them He slowly realized, and McConnell had arrived at the same conclusion, that the spinner simply did not know what he wanted or why. He had measured such properties of his cottons as he could; length and its variance, diameter and form, weight per unit length, and had developed Hughes's technique for determining the breaking load of single hairs. But spinning properties evaded explanation.

McConnell caused spinning tests to be made on the four cottons which the graders in Egypt had rejected. All four were found to be notably good, even samples which looked worthless, and this unexpected result reinforced their joint scepticism as to the value of the expert graders' judgements when applied to unfamiliar cottons. It became evident that actual spinning was the only dependable test and that extensive collaboration between botany and technology was urgently required.

Little Shelford January 1914

Balls proceeded to settle for a time in the village of Little Shelford near Cambridge and there wrote *The development and properties of raw cotton* and *Egypt and the Egyptians*. This latter work was an illuminating study and has more than a passing interest even at the present day. His comments on the psychology of Egyptian officialdom are shrewd and penetrating.

Lancashire 1917-1927

The mutual respect which McConnell and Balls had for each other bore fruit. Balls was invited to start an Experimental Department inside the Fine Spinners, and so began a decade of new activities, beginning with two rooms in Manchester and after three years to a large house in Bollington, Cheshire. The broad outlines of his work are recounted in his book *Studies of quality in cotton*. Much of his time at first was spent in the mills, learning cotton spinning, which, though seemingly odd for a botanist, he succeeded in mastering. The problems for laboratory investigation were many and as he had always held that new problems needed new methods, he lived in a gadgeteer's paradise. By the end of seven years the elementary science of cotton spinning had been outlined and the Experimental Department could invite the constituent mills of the combine to submit difficulties for assistance. By the tenth year, when he resigned to get a change of occupation, this side of the work had by itself repaid everything which had been spent on the Experimental Department including a large green-house in which cotton plants were grown for study.

Planning the Experimental Department was difficult under war and post-war conditions. Two useful devices may be noted. One was the use of Meccano parts to economize time and labour when setting up experimental rigs which could be scrapped or reconstructed properly as occasion demanded. The other was the devising of sectional laboratory furniture, since extensively imitated by others, in which five types of unit could be assembled to make any kind of laboratory or workroom.

The staff at Bollington never exceeded twenty-five, but was rich in variety, including men and women of all sorts, operatives, scientists, engineers, and clerks. These were welded into a team, being all made to feel that they were taking part in worth-while research. A masterly survey of his views of the problems involved in the organization of research is given in an appendix to his book *Studies in quality.* It should be read carefully by all who have to administer research. One idea which he put into practice was to encourage the junior laboratory assistants to work for the external London B.Sc., help in their studies being given by the senior members of the staff.

In the early post-war period, broader issues had developed in the organization of the cotton industry in which McConnell and Balls played a leading part. A Board of Trade Committee grew into the Empire Cotton Growing Corporation and another meeting in Manchester eventually produced the Shirley Institute of the British Cotton Industry Research Association. Balls became chairman for the Joint Standing Committee of both bodies. He envisaged a cotton growing experiment station in Egypt with free interchange of staff between field and industry, but this plan was not realized. Not till many years later did the union of growing and spinning become actual.

Meldreth 1926-1927

He resigned at the end of 1925. He held that there was much to be said for not doing the same job over too long a period. Ten years seems to be about the time when one knows too much about its limiting factors, making difficulties seem insuperable which the novice surmounts in all innocence without undue effort. Ten years of cotton spinning and trouble curing, for him on the wrong side of the Pennines, had made him restless for a change of scene and of interests, so he returned to the Cambridge area and lived inexpensively at Meldreth where there was no more soot and where he rounded off his work by writing *Studies in quality.* Before the problem of what next to do had become acute it was solved by a totally unexpected approach from Egypt asking him to return as head of all cotton work, at the age of forty-five.

Egypt 1927-1947

It was not without anxious consideration that he decided once more to throw in his lot with Egypt, and there for the next twenty years he spent the remainder of his working career. He arrived to find a large unwieldy organization in being with some able workers frustrated by the financial and administrative framework within which they had to operate. He soon saw that for him personal research was no longer possible, but there was ample scope for his great administrative powers. So that although during this closing period his direct contribution to research was slight, the practical achievements were great.

His first major task was to bring the various technical sections into mutual inter-communication, for which he used his old Bollington scheme of monthly meetings over duplicated reports of a Laboratory Research Committee. The work on cotton botany, agronomy and entomology was thus co-ordinated once more. In the preceding ten years expansion had been enormous in accommodation, staff and costs, but this necessarily carried a heavy load of operative and clerical personnel who under the restrictions of government regulations were not particularly efficient. One quarter of the staff employed would have done as much useful work more quickly had they been chosen and controlled under the conditions which he had enjoyed with the Fine Spinners. In a single morning his work might include an investigation involving finger prints on stolen electric light bulbs, lay-out of micro-plot variety trials, struggle to promote a skilled mechanic who lacked a primary certificate of education, and design of apparatus for surveying hardness of soil in the field, and so forth. Underground water still fascinated him and roots were studied by means of glass-fronted root observation chambers three metres underground which Templeton had installed. Survey of water movement all over the seventy acre farm was done intensively and the continuous records of more than a quarter of a century were found to be highly relevant when he came to write his book *The yields of a crop* in 1950 after his retirement.

The concept of pure-line seed supply which he had formerly introduced at Giza had taken root and become almost routine. Large bee-proof cages of stainless steel had been installed to preserve nuclei of pure-seed stocks.

The establishment of a plant for the actual spinning of small samples was done in collaboration with the late H. A. Hancock, who had first worked with him at the age of sixteen as laboratory assistant at Bollington, and then developed into a first-class scientific worker in five more years at the Shirley Institute. This spinning plant enabled hundreds of samples to be spun each year on a 60 gram sample, in no more time than would be taken by hand examination by a grader. The final conclusion of Hancock and Balls was that the micro-spinning test was easier to carry out and more useful than prediction based on routine laboratory tests for hair weight and length, each of which might take half an hour even though done by a skilled technician.

The final discovery that deliberate genetical selection could be done for yarn strength is perhaps the most important achievement yet made in cotton breeding. Carried to a logical conclusion it could result in the extinction of the West Indian Sea Island industry. It is no less important in the control of pure-seed supply since minute deviations in quality due to varietal crossing could be immediately detected and evaluated. It is difficult to assess the magnitude of Balls's own contribution to this crowning practical achievement but it is clear that he was deeply concerned both directly and indirectly, directly through his own inventive genius and indirectly because his attitude to his co-workers was to guide and sustain and seldom to direct.

In the late war Balls's services were used by the forces. In 1940 he became Chairman of the Scientific Advisory Committee to the C.-in-C. at Middle East Headquarters. He devoted much time to the invention of a mine detector. He devised the 'twin circuit' using two search coils with frequencies in the ratio 2:3, giving a striking change of beat note which could locate a sixpence to a millimetre at a distance of a foot. A development of this served to clear the road westward along Africa at the rate of two miles an hour, after the battle of Alamein. A modification of this circuit was redesigned to make it possible to locate metal canisters of narcotics in the stomachs of camels entering Egypt. Loose talk about the success of this device speedily led the ingenious smugglers of opium and hashish to use Bakelite containers.

Retirement at Fulbourne 1945-1960

On his retirement Balls returned to the cottage at Fulbourne, near Cambridge. He was not intimidated by the problems inherent in the gentle art of retiring. He had his workshop and study adjacent, the latter containing all books and files within reach for the first time for twenty years, instead of having them in the wrong country or building when wanted. There were things to make and much to be written. His last book, *The yields of a crop* written during this period, finally brought together in masterly fashion the responsibility of the high water table for the decline in the Egyptian crop, a decline no less real because masked by genetical improvements in seed supply. He busied himself in keeping a record of the first flower dates of all the plants in his little garden which has some amateur gardening interest. One minor routine occupation was provided by his appointment as a co-opted Governor of the Battersea Polytechnic. The courtesy of the Master and Fellows of St John's College to their ex-Fellow—he was given dining privileges—gave him a foothold in Cambridge. He died suddenly on 18 July 1960.

Many honours and scientific distinctions came to him. His election to a Fellowship of St John's College took place while in his first period of service in Egypt, in 1908. Then came Sc.D.(Cantab) in 1917, the F.R.S. in 1928, C.B.E. in 1934, the Honorary Fellowship of the Textile Institute in 1943, C.M.G. in 1944, and the Honorary D.Sc. of Manchester in 1952.

It is not easy to assess his position in the world of science. He did so many things superlatively well in so many branches of knowledge. His own view of himself was objective and modest. He says: 'A decent recognition of limitations and a concentration of attention might have made a proper scientist of me, but Bateson hit a nail square on its head when he reviewed my *Cotton plant in Egypt* for *Nature* in 1912 . . .

' "Had Mr Balls stuck to any of the lines he has begun, no doubt he could have gone much further along it, but as soon as anything like a solution is in sight he would rather start a fresh chase. This is not unfriendly criticism; for many who can follow there are few who can begin, and others will some day make something of the various beginnings here left unfinished." '

Balls was of medium height, somewhat strongly built—a typical blonde East Anglian. He had no small talk but through his wide interests made numerous friends in all walks of life. His manner was quiet and deceptively diffident for he could be a severe critic of loose or shoddy work. He described himself as a scribbler, and indeed he wrote easily and elegantly. As a fellow worker on cotton the writer maintained contact with Balls for more than forty years, and can bear witness to his great personal charm and capacity for deep friendship. In his work he was greatly helped by his wife, who survives him with one son of the marriage. It is fitting to close with what was said of him at the Memorial Service in the Chapel of St John's College. 'We remember before God, William Lawrence Balls, in gratitude for his distinguished work, born of an original and inventive mind, yet spoiling nothing of his gentle simplicity of life.'

S. C. Harland

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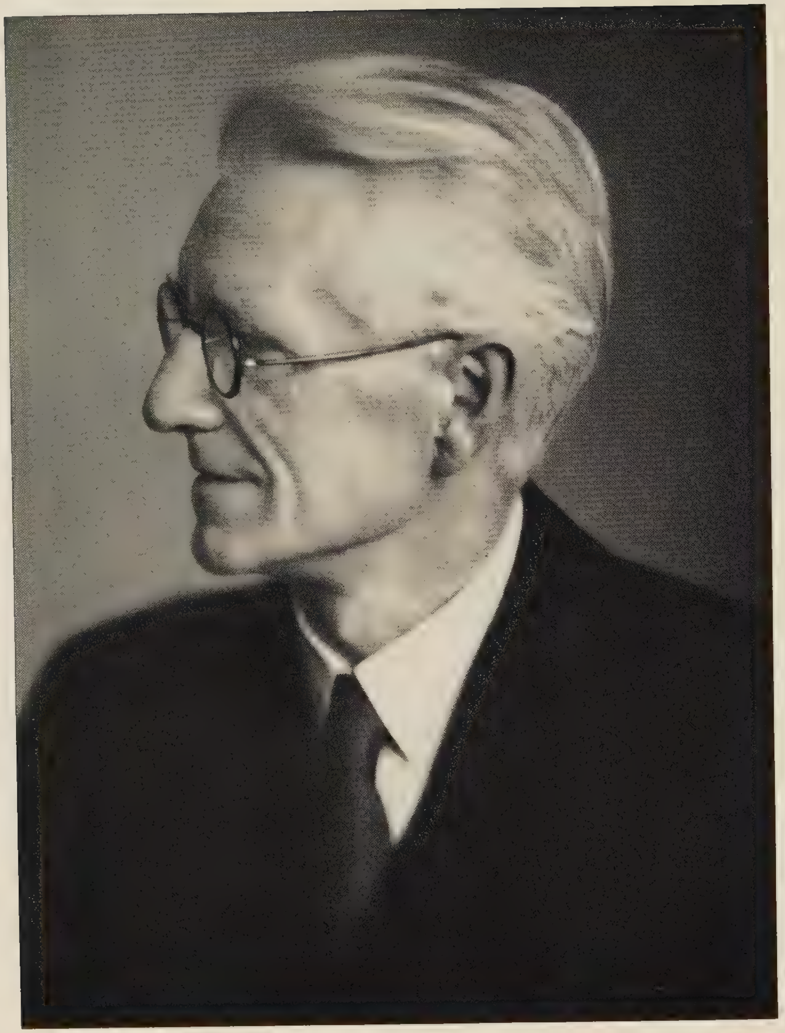

P. G. H. Boswell.

PERCY GEORGE HAMNALL BOSWELL

1886-1960

It was at Woodbridge in East Suffolk, on 7 August 1886 that Percy George Hamnall Boswell was born to George James Boswell Jr. and Mary Elizabeth Boswell, *née* Marshall, of Hobart, Tasmania, the daughter of an emigrant Yorkshire farmer. He was the third of four children, of whom an elder brother and sister died young and a younger brother was three years his junior. The family carried on a printing business in Ipswich, which his younger brother entered in due course, and Boswell relates that he was brought up strictly in a Victorian household on English grammar and punctuation, Charles Dickens and the Bible. His grandfather, a Tory of the old school, did the Party printing in a gorgeous ultramarine and was a stalwart of the Pickwick Club.

As Boswell was somewhat delicate in health, owing to bronchitis and asthma, he was nearly seven years of age before he entered St Matthew's Church of England School, whence in 1896 he gained an Ipswich Scholarship to the Higher Grade School at Ipswich, where he remained until 1901. He noted that discipline was very severe, but the teaching very good and to his delight the pupils were 'soaked in the Bible and Prayer Book English' and in 'beautifully phrased collects, psalms and gems of Bible literature, songs and laments' many of which he remembered, and used, to the end of his life. In 1899 the school moved to new buildings which were excellent, with splendid laboratories. The Headmaster, Pollard Wilkinson, was a scholar and a man of broad views. His son, George, became Boswell's best friend and the latter much appreciated the access which this brought him to Pollard Wilkinson's fine library and home. George Wilkinson later became a recognized expert on aero-engines. At school, as an alternative to sport, the boys were taken for country excursions, when they were taught elementary surveying and practical trigonometry; they also visited chalk pits to collect fossils, which they later identified in the Ipswich museum.

Although no other member of the family appears to have had any particular taste for science, Boswell early seems to have developed such an interest, at first, however, confined to chemistry and physics. He showed an original bent by attending evening classes at the Ipswich Technical School under the Science and Art Department of the Board of Education, a procedure so irregular for a boy still at school that when the Board's inspectors found out, they promptly stopped it. From this experience, however, the youthful Boswell gained his first knowledge of astronomy, physiology, biology and sculpture; in particular his curiosity regarding geology was awakened and this took him walking and cycling to numerous fossiliferous Crag deposits and chalk-pits in East Anglia.

The new vista which opened up to the young man was evidently at variance with the established views of the family for he notes that he disagreed, though he did not quarrel, with his family and decided to leave home and earn his own living as a teacher. So we find him from 1902-5 as a Pupil Teacher, first in a Public Elementary School which he disliked intensely, chiefly because of the extreme poverty of the children, the conditions and the terrible, vitiated atmosphere which gave him a great craving for fresh air. In 1903 he was a founder member of the Ipswich and District Field Club and became its first Honorary Secretary and editor. He matriculated at London University in 1905 and continued to teach at the Technical School at Ipswich until 1912. During this time, however, he turned to field geology, at first unaided but later encouraged by the enthusiasm of a school-master who came to Ipswich from the North country—George Slater—later to be a life-long friend, and also by correspondence and ultimately by personal contact with F. W. Harmer, W. Whitaker and A. Vaughan. His investigations were directed first to the Pliocene faunas and then to the stratigraphy of the Eocene and Pliocene of Suffolk and Essex, the beginnings of researches which were later to be pursued at Imperial College and some of which were published in the *Quarterly Journal of the Geological Society*.

His early geological work was carried out in the intervals between school-teaching, at weekends and often far into the night, largely guided, as he recalled, by Watts's little book *Geology for Beginners*. A year or two later Boswell plucked up courage to write to Watts on some point which perplexed him and thus started a long friendship which lasted until Watts's death in 1947. Unfortunately, as a result of excessive reading and peering down a microscope in artificial light he developed choroiditis in both eyes, and at the age of 18, in 1904, he went blind for several months and had to be led about and read to. The retina of his right eye was irretrievably damaged and never afterwards was he able to see stereoscopically, indeed he was frequently handicapped in later life by recurrent eye-trouble.

In 1906 he was elected a member of the Geologists' Association and in 1907 a Fellow of the Geological Society at the age of 20, which was illegal as he was not of age. He and his sponsors acted in innocence! Shortly afterwards the age of candidates had to be declared on their proposal forms. In the same year he attended his first British Association Meeting at Leicester, and in 1910 he made his first close contact with Imperial College, at a course in higher pure and applied mathematics for science teachers. He was working steadily for his degree and in 1911 took a First Class B.Sc. (External) degree at London University in geology and pure and applied mathematics.

At the comparatively advanced age of twenty-six Boswell became a full-time student at the Imperial College (Royal College of Science and Royal School of Mines), being particularly attracted by the prospect of working under Professor W. W. Watts, F.R.S. who had already given him much encouragement, and to whom he records that he owed an immense debt of gratitude.

Launched on his career as a geologist, Boswell went from strength to strength. He had entered Imperial College as the recipient of a Remanet Studentship in 1912, graduated B.Sc., Hons. (London) and Associate of the Royal College of Science, both First Class, in 1913, the year in which he was awarded the Clement le Neve Foster Prize. He continued as a Research Student until 1915, gaining the D.I.C. in 1914 and the D.Sc. of the University of London in 1916. During the former year he founded and became the first Honorary Secretary of the Students' De la Beche Club at Imperial College. Although his geological research was still concentrated in East Anglia, the emphasis had shifted somewhat to include not only the stratigraphy of the deposits of that part of England, but also the underground structure, water supply, the geomorphology and in particular the glacial deposits. This research was of such quality and promise that in 1914 the Geological Society of London awarded him the Daniel Pidgeon Fund and in 1917 the Balance of the Proceeds of the Wollaston Donation Fund.

At that time his investigations were a continuation of his earlier work on the Eocene and Pliocene of Suffolk and Essex, and involved studies of Chalk zones, upon which little had been done, and then an assessment of the magnitude of the Eocene unconformity in the London Basin. This brought him into touch with A. W. Rowe, of Chalk-zoning fame, and it was in connexion with these researches that he first introduced into English in the form '*isopachyte*' the term de Geer had used when describing the Swedish Pleistocene.

A far-sighted aspect of this work was the attempt to discover whether or not the detrital mineral assemblages of the sediments were distinctive of particular horizons and could be used over limited areas for purposes of correlation. This led naturally to the investigation of a wide range of British formations and Boswell acquired a very large, and in those days a unique collection of samples of British and overseas sedimentary rocks. The detailed study of the detrital minerals of rocks was at that time somewhat neglected even though investigations by W. Mackie, T. Crook and H. H. Thomas had demonstrated that many British formations contained considerable varieties of such minerals. In this little-worked field Boswell found that of some 160 minerals that occur as significant constituents of crystalline rocks practically every one appears also as a detrital constituent of clastic rocks, and that particular varieties are often diagnostic of mineral provenance. In many cases new light was thrown on the palaeogeography, tectonics and climatic conditions existing at the time of sedimentation. The qualitative and quantitative aspects of this study were later to prove of great economic importance in the War years and in the following decade.

At the same time, as he grew more familiar with the Glacial deposits of Suffolk and Norfolk Boswell became deeply interested in the problems of the correlation of these beds and their relation to the culture stages of Early Man.

From 1915 to 1917 Boswell was a Demonstrator in Geology at Imperial College, but a further important turn was given to his researches by his appointment in 1916 as Scientific Adviser to the Ministry of Munitions. In this capacity he served until 1919 as Geological Adviser to the Departments of Scientific and Optical Glassware, Iron and Steel Production, Potash Production and Mineral Resources generally. In 1918 he was attached to the British War Mission in the United States of America. This work involved him in the detailed examination of a wide range of sediments and sedimentary rocks and stimulated his studies of particular minerals. Much of this research was made available in a range of scientific papers and books, including *A memoir on British resources of sands suitable for glass-making*, published in 1916, with a supplement issued in 1917, and a second edition in 1918, as well as *A memoir on British resources of refractory sands for furnace and foundry purposes*, also in 1918.

The subject of 'Sands; considered geologically and industrially, under War conditions' formed the title of Boswell's Inaugural Address on his appointment as George Herdman Professor of Geology at the University of Liverpool in 1917. This marked the commencement of his close association with Liverpool which lasted for thirteen years and began what many consider to be the finest period of his long and varied life. Until the War was over in 1918, in which year he was awarded the O.B.E., his energies were still largely employed in researches for the Ministry of Munitions, but he then set to work wholeheartedly to build up at Liverpool a department of geology of the highest repute. Aided first by C. P. Chatwin and later by E. Neaverson and I. S. Double, old friends of his Suffolk days, he attracted to Liverpool a group of students compounded of ex-service men and youngsters straight from school. This enthusiastic yet critical body of young geologists engaged in such varied researches as sedimentary petrology, the volcanic rocks of Snowdonia and the Lake District, the Silurian and Carboniferous of North Wales and the Triassic rocks and glacial deposits of the Liverpool district. Inspired by the leadership of their Professor and his staff, they were trained to occupy in due course a variety of senior posts in British and overseas universities, in the Geological Survey of Great Britain, in museums, overseas surveys and in mining and oil companies. Encouraged by Boswell a number of his research students proceeded to his *alma mater*, thereby strengthening the ties between Liverpool and the Imperial College which have remained close ever since. Boswell was unsparing in his efforts. He continued to be deeply engaged in his researches into sediments and sedimentary rocks, topics which formed the subject of his Presidential and other Addresses to the Liverpool Geological Society and his Presidential Address to the Liverpool Biological Society, later to be collected together in the form of a book. It is interesting to realize that at about this time Boswell concluded that existing techniques would not serve for satisfactory investigations of the petrology of sedimentary minerals. Indeed it was not until much later that X-ray diffraction, the electron microscope and other technical methods enabled geologists to pursue such studies with a reasonable measure of profit.

In the course of his work on sediments he carried out experiments at Liverpool on the deposition of sandy and finer materials, and served on the scientific committee set up by the Mersey Docks and Harbour Board to investigate the causes of silting up of parts of the Mersey estuary. This led, through his acquaintance with F. G. Donnan, a member of the committee, to a meeting with Herbert Freundlich (a refugee from Nazi oppression) who was given hospitality by Donnan at University College, London, and there carried out tests on the thixotropy and dilatancy of some geological materials. Boswell was much impressed by this work which led him to realize its practical significance to oil-geologists and to civil engineers interested in soil mechanics, a subject to which he devoted much thought and work.

Thus he concerned himself with experiments on the physical characters of natural sediments, introducing the term 'rheotropy' to cover phenomena such as thixotropy, false-body and the rest–hardening of clays. Other fields of investigation included deep sea deposits (the term 'hypedaphic' was introduced by him), co-precipitation and the salt content of connate water in sediments. Doubtless his most important contribution to the petrology of sedimentary rocks was his recognition of the significance of the relative stability of their mineral constituents in geological time and under varying physical conditions. These problems led him in turn to study post-depositional changes and the development of authigenic species. Thus he emphasized the life-histories of sediments rather than their post-mortem records. With the development of the new techniques of the present day this change in viewpoint has become all-important.

In the early 1920's Boswell turned his attention to the stratigraphy and tectonics of the Lower Palaeozoic rocks of North Wales, a study which he had started earlier as an antidote to his Tertiary work, but which had been interrupted by the War. He was a confirmed admirer of C. T. Clough, his model of a field geologist, and he was insistent, at a time when not all university schools of geology were equally convinced, on the fundamental importance of first class field mapping.

Despite the strain of teaching and planning and organizing a new and rapidly growing department, and the claims of his own researches, he yet found time to take a very active part in the life of the University, being Chairman of the Science Faculty, the Men's Hostel, University Press and Library committees, besides serving as staff representative on sundry students' organizations.

Boswell was a keen mountaineer, delighting not only in the Welsh mountains, but, in company with his friend Emil Montag, the Swiss Consul in Liverpool, and at times accompanied by one of his senior students, engaging in strenuous mountaineering in the Swiss and Austrian Alps. He was elected a member of the Alpine Club and travelled widely, visiting Europe, America, India, East and South Africa.

His love of Switzerland and his friendship with Swiss geologists, in particular the great master Albert Heim, doubtless inspired him to agree to translate

F. Heritsch's *The Nappe theory in the Alps*, a task which he felt to be well worth while because many believed that Collet's book on the Alps had over-emphasized the Nappe Theory and that of Continental Drift.

At about this time also, there was conceived and carried out the remarkable Mersey Road Tunnel, linking Liverpool and Birkenhead. Boswell was appointed Geological Adviser to the scheme and served as such from 1925 to 1933, greatly assisting the engineers in the course of the work, besides contributing to the Liverpool Geological Society valuable papers on the Glacial Drift and Trias revealed during the operations.

While at Liverpool he served for several years on the Council of the Geological Society of London, a duty which involved much travel to and from meetings. As an authority on East Anglia he complied with Flett's request to write three memoirs for the Geological Survey, and he also contributed articles to the *Encyclopaedia Britannica*, to *Regionalen Geologie* and to the *Handbook of the Geology of Great Britain.* In 1927 he was awarded the Liverpool Geological Society's Silver Medal and in 1929 the Geological Society of London awarded him its coveted Bigsby Medal. Other honours conferred on him were the Concrete Institute's Medal in 1920 and the Charles Kingsley Memorial Medal in 1925; moreover he was a founder member and later an Honorary Fellow of the Society of Glass Technology.

These tremendous exertions were not without their penalty, for though extremely energetic and gifted with enormous drive, Boswell was not entirely fit. He suffered periodically from attacks of asthma and bouts of headache which caused him considerable inconvenience and discomfort.

In 1930, on the retirement of W. W. Watts, Boswell returned to his old college as Professor of Geology and Head of the Departments of Geology, Mining Geology and Oil-Technology. There he was faced with a task of organization which claimed a large part of his energies. In this work he was aided by a number of valiant lieutenants, including V. C. Illing, F.R.S. in Oil-Technology, C. G. Cullis and later W. R. Jones in Mining Geology. A. Morley Davies in Palaeontology, A. Brammall, G. Slater and later F. G. H. Blyth in Civil Engineering Geology and David Williams in Applied Geophysics. This team maintained the geological traditions of Judd and Watts, providing a combination of sound teaching and a wide range of research, a system which continued to furnish a large number of well-trained men to fill many posts of importance in academic, survey and company work.

All this did not, however, prevent Boswell from continuing his researches, particularly into the Silurian rocks of North Wales, a great task which is recorded in a number of papers, mainly in the *Quarterly Journal of the Geological Society of London* and in the *Proceedings of the Liverpool Geological Society*, some of the latter being written jointly with I. S. Double. All were ultimately brought together in a large volume entitled *The Middle Silurian Rocks of North Wales.* This was a true labour of love, extending over a period of more than 25 years, a piece of work which will undoubtedly stand the test of time, despite doubts thrown on certain structural interpretations by objectors who did not always express themselves in the most tactful manner.

In 1931 Boswell was elected to Fellowship of the Royal Society. Ever since the Leicester meeting in 1907 he had been an ardent supporter of the British Association for the Advancement of Science and it was perhaps no surprise, therefore, that he was appointed a General Secretary in 1931, serving as such until 1935 when he became General Treasurer until 1943. While in office he interested himself especially in the maintenance of Darwin's home, Down House, a trust of the Association. Particularly devoted to Section C, he was elected its President for the York meeting in 1932. Since from early days he had been a zealous student of Pleistocene geology, it was natural that he should choose as the subject of his address 'The contacts of geology: the Ice Age and Early Man in Britain'.

In the same year Gowland Hopkins, then President of the Royal Society, asked Boswell to go to East Africa, with the aid of a grant from the Royal Society, to report on the geology of the sites and the validity of the claims of L. S. B. Leakey's Expedition to have found in Kenya and Tanganyika skeletal remains of Early Man which would have carried the date of the arrival there of man of modern type back into Early-Middle Pleistocene. Boswell had already been to Kenya under Leakey's guidance in 1929 and was in touch with E. J. Wayland and others who were studying the successions in East and South Africa. It was found that the sites (at Kanan and Kanjera) had not been properly marked and could not be re-identified. Doubt about the age of the human material therefore remained and Boswell was compelled to put the claims into a suspense account.

In 1932 Boswell was elected a Secretary of the Geological Society of London, in which capacity he continued for two years. Always interested in water-supply problems, in 1934 he was appointed Geological Adviser to the Metropolitan Water Board and so served until 1953. In that capacity he prepared comprehensive reports on the wells in the Kent northern and southern areas, following an exhaustive investigation of the underground resources within the area of the Board's activities. During his period of service Boswell was much involved in the work of preparing and promoting water-supply schemes in the London area and gave evidence before the House of Lords Committee on the 1944 Bill and the Departmental Inquiry in 1947. His evidence at the latter inquiry was embodied in his *Review of the resources and consumption of water in the London area* which is generally regarded as the most authoritative work on this topic. His careful preparation of evidence and clarity of exposition earned for him a great reputation as a witness.

In 1935 he was President of the Conference of Delegates of Corresponding Societies of the British Association. The continuance of his interest in matters prehistoric is testified by his election in 1936 to the Presidency of the Prehistoric Society.

By 1938 it was evident that the strain of administration and research work was telling severely on his health and he was compelled to resign from the chair at Imperial College, the title of Emeritus Professor being conferred on him by the College before his retirement.

In 1939 Boswell married Hope Dobell, the sister of the late Clifford Dobell, Sc.D., F.R.S. and so began a very happy partnership with one who loved and understood him so well. Release from academic responsibilities enabled him to tackle new tasks and in 1940-41 he accepted office as President of the Geological Society of London, and during the 1939-45 War he served on several departmental committees, busying himself meanwhile with many applications of geology to industrial and engineering problems. He had been appointed a Governor of the Imperial College in 1936 and continued as such until 1949, besides serving on the Councils of the Royal Society, the Geological Society of London, the Geologists' Association, the Mineralogical Society and the Palaeontographical Society. He was elected a Foreign Correspondent of the Geological Society of America and of the Société Géologique Belgique and an Honorary Member of the Geologists' Association, the Institute of Quarrying, Refractories Association, Royal Geological Society of Cornwall and the Suffolk Natural History Society. Unfortunately his health continued to be but indifferent and he had increasingly to take care and to seek a mild and dry climate. His interest in geological matters was in no degree abated and he concerned himself particularly with problems of water supply. Another major interest was a continuation of his work on sedimentation, especially in the study of thixotropy and allied phenomena; he continued to publish papers on these subjects until 1955 and was working to the end on a treatise on muddy sediments, to be published posthumously.

During the later years of his life he became increasingly an invalid, devotedly nursed by his wife, but maintaining until the last a keen interest in matters geological and in the doings of his old students. He passed away at Ruthin, North Wales, on 22 December 1960, at the age of 74, his ashes being scattered on his beloved Great Orme.

Such is the brief summary of the life of P. G. H. Boswell, but its recital tells comparatively little of him as a man. He was rather above middle height, lean and with somewhat sharp features, but always with a merry twinkle in the eye behind his glasses, and around his mouth there played an almost perpetual smile. His hair in middle age was light, almost sandy in colour. It is worthy of note that he was naturally left-handed and successfully resisted early attempts to alter this. He generally used a hammer in his left hand, but in many ways he was, or became, ambidextrous. He was an excellent lecturer and possessed the gift of inspiring the young with the wonder and importance of his subject. To his students and many friends, to whom he was affectionately known as 'P.G.H.' or 'Bozzy', he was an inspiration and a guide, never sparing himself in his own labours and ever ready to advise and guide others in their researches, whatever their chosen field. His own preferences were mineralogy, stratigraphy, structure, Pleistocene geology and the economic aspects of the science—he made no claims to be an expert palaeontologist. Despite his addiction to geology, Boswell always took a keen interest in the English language, being greatly concerned with the correct use of the written and the spoken word; he deplored the misuse of words by scientists and others alike. He was impelled by the idea of service, to the individual and to the state. Perhaps he will be best remembered by his many students and friends for the active middle period of his life. Then will be recalled the companionship he displayed when, during his occasional off-duty spells, he would show his true self. The happy days when he accompanied the students' annual row from Chester to Eccleston Ferry, the relaxation on the greensward and the journey back after tea, must have been experienced to be fully appreciated. Memories of tolerance and wise handling of student pranks on field parties and his insistence that hard walking and long treks were the way to counter high spirits, can be matched by recollections of his feats as a mountaineer in the Alps; his real satisfaction on achieving some high peak; and on one occasion his keen disappointment when, on the advice of the guide, the party was forced to abandon the attempt to reach the top of the Finsteraarhorn in a gale when only a few hundred feet short of the summit.

Haec olim meminisse juvabit.

In writing this memoir I have been greatly assisted by Mrs Hope Boswell, Mr Ivan S. Double, Professor David Williams and Dr John C. Harper, to whom I return many thanks.

G. H. MITCHELL

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Broglie

MAURICE, LE DUC DE BROGLIE

1875-1960

The family of de Broglie takes its name from the pleasant little town in Normandy, on a hill above which their château stands and where another great man of science, Fresnel, was born. Maurice, Le Duc de Broglie, was born on 27 April 1875, two years before his grandfather, a distinguished historian and statesman and member of the Académie, formed that cabinet at the head of which he brought about the only dissolution of the Chamber of Deputies that has ever happened. He came of a rather famous family which, between the 17th and 19th centuries, gave France several ecclesiastics, two Marshals of France and two Prime Ministers. Prince Louis de Broglie, one of the greatest living mathematical physicists and a member of the Académie des Sciences, is a younger brother of the late duke.

Maurice de Broglie was a brilliant pupil at the Collège Stanislas and later (1893) entered l'École Navale. He served in the French navy for eleven years and consequently received the splendid mathematical training which is given to its officers. He qualified as 'professeur des hautes études' and in 1924 became a member of the Académie des Sciences in which, as is well known, are many very distinguished mathematicians and physicists. He was greatly interested in X-rays and rays from radioactive substances and his work won for him many honours, not only from his own country, but from England and other countries. The University of Oxford conferred the D.C.L. on him.

After leaving the navy he installed a small laboratory at his residence in the rue Châteaubriand. Here he studied the ionization of gases and small electrically charged particles and the movement of charged atoms, molecules and of *very large* charged particles—large compared with molecules—such as, for example, those in tobacco smoke. This work formed the subject of his doctoral dissertation in 1908.

During his career in the navy he naturally became interested in the scientific problems associated with it and in dealing with them later he found it necessary to enlarge the laboratory in the rue Châteaubriand by adding to it the higher floors in the building.

After the enlargement of his laboratory he had many students and disciples and it is said that he dealt with them kindly and patiently.

He was, it appears, the first to examine charged particles in an electric field with the ultra-microscope. His observations revealed to him that small particles become charged when exposed to X-rays. The experimental work of Rutherford drew his attention to nuclear physics and he anticipated some of Millikan's discoveries. He also carried out observations on cosmic radiation, using a Wilson cloud chamber and the great electro-magnet of the Académie des Sciences, the most powerful then existing and learned that it consisted mainly of positively charged particles. He appears to have been the first to study spectra with a revolving crystal and he verified Moseley's results. No doubt, Louis de Broglie profited by the experimental training he received from his brother.

In 1934 he was elected a member of l'Académie Française.

During the war of 1914 Maurice de Broglie was mobilized as a naval lieutenant and contributed in important ways to the detection and localization of the enemy's U-boats. After the war he studied, with his pupil Alexandre Dauvillier, discontinuities in absorption and mass-spectra and published (1922) an admirable work, *Les rayons X.* He was appointed secretary of the Congrès Solvay in 1911 and his brother Louis developed a lively interest in the problems then studied and discussed. There is no doubt that this young theoretical physicist benefited very greatly by his contact with Maurice's experimental work and the Conseil Scientifique de l'Energie Atomique was largely guided by him. He appears to have been the first, in France, to use a Wilson cloud chamber. Mme Curie was interested in his work. He was able, with his Wilson cloud chamber to estimate the enormous energy found among the particles of cosmic radiation and to show that these particles were all, or most of them, positively charged. He installed a Cockroft-Walton generator of 300 000 V.

Perhaps the greatest contribution of Maurice de Broglie to physical science was the training of so many clever young men to be his disciples and fellow workers.

Just recently the Cardinal Eugène Tisserant was elected to the Académie Française. He assumes the chair made vacant by the death of the Duc de Broglie and becomes the 38th member of the academy.

I wish to acknowledge my indebtedness to the necrologue on the Duc de Broglie addressed to the Académie des Sciences by M. Louis Leprince-Ringuet.

WM. WILSON

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W. J. Duncan.

WILLIAM JOLLY DUNCAN

1894-1960

William Jolly Duncan was born on 26 April 1894 at Hillhead, Glasgow. He was the son of Robert Duncan, Senior Partner of Ross & Duncan, Marine Engineers and Boilermakers, Whitefield Works, Govan, Glasgow, who died in 1924. His mother, Mary Anne Jolly, was a daughter of William Jolly, H.M.I. He had two sisters, both younger than himself. He was married in September 1936 to Enid Meyler Baker, daughter of G. S. Baker, O.B.E., D.Sc., M.I.N.A., who was for many years the Superintendent of the William Froude Laboratory, National Physical Laboratory. There were four children of the marriage, all daughters. Duncan died on 9 December 1960, some little time after undergoing a severe operation, from which he appeared to be recovering, but then suffered a fatal relapse.

Duncan was very reserved and seldom spoke about his own life, and particularly about his early years and the influences which led him to a scientific career. Even his wife was not at all well informed on these matters, and Duncan's early history and personal feelings would have remained largely a blank had it not been for the fortunate fact that he left a fairly full account of the first half of his life, an account which came to light amongst his papers after his death and the existence of which was not known, even to Mrs Duncan, until then. The account of Duncan's earlier years which follows is very largely a transcript of his own words. The chief formative influence during his early childhood, in relation to his scientific career, was his father, who was a graduate in engineering and had himself been much influenced by the Thomson family, having sat under Professors William and James Thomson and enjoyed close friendship with James Thomson Jr, the son of Professor James Thomson. Duncan's father, as an engineer, naturally turned his son's mind towards engineering and the physical sciences on which it is based. He did this, not by formal instruction, but in the course of conversation and by giving his son model engines, boats, and such like. Duncan's maternal grandfather, another William Jolly, and his mother encouraged his interest in natural history. His earliest interest of a scientific nature was in wild flowers, an interest which persisted throughout his life, though he never claimed to have become an expert botanist. The lovely wild flowers of the Ayrshire coastal region, and in the neighbourhood of Dalchonzie, where the family went for long summer holidays, gave him great delight, and he augmented his own observations by poring over the illustrated volumes of Anne Pratt's *Flowering plants*. This may be a convenient place to say a word about Dalchonzie, an estate which belonged to Duncan for a number of years after the death of his father, and to which he went whenever he had the opportunity. It lies about two miles west of Comrie, on the southern bound of the Highlands, and is a large house, with a farm nearby, and a good deal of wild land. Ben Vorlich is seen well in the distance, and there is even a sizeable hill on the estate itself. The surrounding country, especially to the westward towards Loch Earn, is very beautiful, and the peaceful quiet of the place must have been a great incentive to creative thought during respite there from the more arduous affairs of daily life. It must also have been a great sacrifice for Duncan, when, later in life, he felt obliged to give up this delectable retreat.

At about the age of ten Duncan became fascinated by chemical experiments and read parts of a *Manual of chemistry* by Fownes, which was in the house. The stenches which he made in the nursery were such that his father decided to build a shed in the garden of the Glasgow home to serve as a laboratory. Two of Duncan's uncles were analytical chemists who had emigrated, leaving much chemical apparatus and chemicals at the home of their father in Blantyre. Thither Duncan would go by rail on Saturdays and return triumphantly with the 'loot' he had been allowed to take away. He so became possessed of many dangerous chemicals, but apparently, even at so early an age, had enough knowledge of them not to harm himself or anyone else. His interest in chemistry was encouraged by Mr W. J. Brough, who acted as tutor to the children during long summer holidays at Dalchonzie. Duncan remarks that, at this time, all his pocket money was spent on chemicals and apparatus. Until the age of fifteen he took little interest in physics, which seemed to him to be a dry subject. Although he was good at mathematics and interested in geometry, he had no great enthusiasm until he was introduced to the calculus. 'But then, what a transformation!' he writes, 'Soon I thought of little but this wonderful calculus that enabled one to do easily so many difficult things.' There was the impulse which led, in later years, to Duncan's very considerable mathematical ability.

Apart from a few weeks at Bellahouston Academy, terminated by whooping cough, Duncan was educated at home by a governess or tutor until he was ten years old, when he was sent to Allan Glen's school in Glasgow and was placed in the lowest class. This school had a high reputation, more especially for scientific and technical education. He remained there for two years and made satisfactory but not brilliant progress. The family then moved to London. This was presumably because Duncan's father was elected M.P. for Govan at about this time. Duncan was then sent to Dulwich College as a boarder and was placed in the First Form and in the Third Mathematical Set in the Junior School. It is interesting to note in passing that Duncan's father sought the advice of Sir William Ramsay on a boarding school for his son, and was told that Clifton or Dulwich were the best. Since Dulwich was the more conveniently situated the decision was made that he should go there. In his first year he did well, but not brilliantly, but in the summer term, 1907, he had a severe breakdown of health during a holiday at Dalchonzie after catching measles at school. It was not until January 1908 that he was well enough to resume schooling and then he went to St Hugh's Preparatory School, then at Chislehurst and later at Bickley, for two terms. He was greatly inspired by the Headmaster, the Rev. J. F. Johnson and his brother Mr A. S. Johnson, who did much to improve his knowledge of languages. He confessed, in later years, that he had never been more than mediocre at languages but that he was always intensely interested in words. One can well believe this, in view of the lucidity and precision of his writings. He returned to Dulwich in September 1908 and was placed in the Modern Upper Third, where he at once began to take first place in the form. He was also in the senior Mathematical Set of the Junior School, conducted by a Mr Rumsey whom he found to be an 'able and jocular teacher'. Mr Rumsey was given to inventing nick-names for his students, and his name for Duncan was 'Drunken', surely a most inappropriate one, as there can have been few more sober!

It was Rumsey who proposed to Duncan's father that he should go into his Mathematical Form with a view to studying for a scholarship at Cambridge, but his father decided against this course as he wished his son to succeed him in his engineering business and not to take up a scientific career. Dulwich College, at that time, had a separate engineering side to the Senior School, headed by Mr F. W. Russell, and Duncan joined this in 1909. He progressed steadily and in 1910 he passed the London Matriculation Examination following this by passing in 1911 the London Intermediate (Engineering) Examination as an external student. During this last year at Dulwich, Duncan was a School and House Prefect and in the Sixth Form. Mr Russell then left him much to his own devices and one of the books he read on Russell's advice was Drysdale's *Foundations of alternating current theory*. He had a very high opinion of the education he received in the latter part of the Dulwich period, especially in physics, and thought that the practical work in the physics and mechanics laboratories was of outstanding value to him. He also continued his early liking for chemistry by reading most of the books in Ramsay's series *Text books of physical chemistry*. On leaving Dulwich College he was awarded, without having made any application, a 'Leaving Exhibition' by the College. Duncan remarks that although his memories of school years are pleasant, he did not really enjoy his schooling until the last two years when he was high up in the school. He attributed this largely to imprisonment within close bounds, as he was not allowed to go more than half a mile from the College and could not enjoy country walks, such as he loved at Dalchonzie, nor visit interesting shops. Moreover, he was always useless and uninterested in games.

In October 1911 Duncan became an engineering student at University College, London, and as he had already passed the Intermediate Examination, went straight into the Second Year. His principal instructor was Professor J. D. Cormack, later Professor of Engineering in the University of Glasgow, whom he found to be a very sound, able and pleasant teacher.

He missed Professor Karl Pearson's lectures on graphics, as Pearson had just left to become Galton Professor. However, he had the benefit of Pearson's notes, which formed the basis for a set of lectures given by Mr Sprague. He also benefited from teaching by Mr Goudie, later Rankine Professor at Glasgow, by Mr H. P. Philpot, Professor J. A. Fleming and Mr W. C. Clinton who later succeeded Professor Fleming. He recalls that he found Mr Dibb, Lecturer in Mathematics a very friendly teacher, who regarded him as something of a prodigy, 'an engineering student actually interested in mathematics!' Duncan sat for his degree in the summer of 1913 and obtained First Class Honours. In order formally to qualify for the internal degree of B.Sc.(Eng.) he was required to remain at University College for a third year. He was also awarded the Engineering Diploma of the College, with Distinction, and the A. P. Head Memorial Prize, described as 'highest engineering award'. During this third year Duncan had been doing independent and original work in kinematics and the theory of roulettes, and wrote a monograph on the latter subject which was shown to Dr Whitehead, who endeavoured to have it published by the Cambridge University Press. However, Professor E. W. Hobson, to whom it was submitted, decided against publication, but suggested that Duncan should write some six independent papers covering the principal results of novelty. Unfortunately he could not do so, on account of the war of 1914-18, and this early research work of his never was published except for a paper on 'A graphical method of finding inertia forces' which appeared in the *Journal of the Institution of Mechanical Engineers* in March 1915, and was his first published paper. He continued to work on these subjects in later years, but apparently never published anything further on them.

At this stage in Duncan's career, just as he was ready to begin to apply his knowledge, the first world war broke out, and he had to abandon academic activity for a time to render military service to his country. He served in the Royal Army Service Corps in France and Flanders, but was later attached to the Aeronautical Inspection Department, where he presumably found some opportunity to use his engineering knowledge. When the war ended he joined his father's firm of Ross & Duncan, Marine Engineers, of Glasgow and remained there until 1926. During this time he continued independent work in his spare time, filling a large number of notebooks but producing only two published papers, both on engineering subjects. He became an Associate Member of the Institute of Mechanical Engineers in 1920, and a partner in the firm in 1922. The firm of Ross & Duncan was voluntarily wound up in 1926, in common with several other firms in the marine engineering business, on account of the very severe and prolonged slump in trade. With the help of a recommendation from Professor J. D. Cormack he secured an appointment at the National Physical Laboratory as a Junior Assistant with a commencing salary of some £330 per annum. This, at the age of thirty two, with a first class degree, and after seven years in the engineering industry, reflects how poor was the money paid for scientific research in those days, even allowing for the greater purchasing power of the pound as compared with that today. Duncan was allotted to the Aerodynamics Department, of which I was then Superintendent. The first piece of work on which he was engaged was the recalibration on the whirling arm of the standard Pitot-static tube, in collaboration with Mr F. C. Johansen, who later became better known for his classical experiments on the wind resistance of railway trains. This Pitot-tube calibration appeared at first sight to be a simple matter, but it proved very difficult to get the desired accuracy of about one part in a thousand on account of the large and irregular swirl set up by the whirling arm. In December 1926 Duncan became the collaborator of Mr R. A. Frazer (later Dr and F.R.S.) in his work on the flutter of wings and other parts of aeroplanes. They were soon joined by Mr A. R. Collar, now Professor of Aeronautical Engineering in the University of Bristol, and this team of three produced fundamental solutions of flutter problems which have become classical and are the bases of all subsequent flutter investigations. This basic work was described in two monographs (*Reports & Memoranda of the Aeronautical Research Council*, Nos. 1155 and 1255). The former of these reports has often been somewhat irreverently called 'The Bible of flutter'. It is exceedingly difficult to apportion the credit for this outstanding research to the three members of the team, who worked together very closely throughout its development. Perhaps it might be said that Frazer was primarily the mathematician who formulated the general theory of flutter, and that at first Duncan, and later Collar, were the experimentalists who measured the aerodynamic derivatives involved, and devised many ingenious experiments to check the theory. But before long both Duncan and Collar became deeply involved in the mathematical side of the problem and made invaluable contributions to it, though Frazer, who has been called the 'Father of flutter', was undoubtedly the ablest mathematician of the three.

The flutter problem had so great an influence on Duncan's subsequent research that it is perhaps not out of place to give a brief history of the early stages of the investigation. The problem became of great importance about this time on account of accidents to aeroplanes which were obviously attributable to a violent oscillation of wings or controls which led to structural failure. The question was, what caused these oscillations to build up suddenly in amplitude, and what means could be found to prevent the phenomenon from occurring in the future? Frazer started by considering the equations of motion applying to such oscillations, in terms of the aerodynamic derivatives involved and the inertial and stiffness characteristics of the structure. Needless to say, these equations were very complex, since the mode of deflection was a function of the aerodynamic loading and therefore of the wind speed, and a complete analytical solution was not found possible. The problem was greatly simplified by the concept of the semi-rigid model, in which, for instance, the complex bending and twisting of an actual wing were approximately represented by the flapping and torsion of an otherwise rigid aerofoil. This, with the assumption that the modes were independent of wind speed, and considering only the fundamental, reduced the number of parameters required to describe the motion and led to solutions of a tractable kind. These were checked against experiments in the wind tunnel, in which rigid aerofoils were mounted on springs, or had two spars hinged elastically at the roots, so as to give the two degrees of freedom above mentioned. Models of actual wings, but with much reduced elasticity so that they would flutter at the comparatively low speed of the tunnel, were also built and tested and it was thus possible to assess the degree of approximation attained in the simpler semi-rigid approach. Duncan first proposed the use of normal modes to describe the deflexions, as a better approximation than the semi-rigid one, and this method is now almost universally used. The problem was much more complicated when a control surface such as the aileron entered into the motion, since the semi-rigid approach now involved three parameters instead of two, the third being of course the motion of the aileron about its own hinge. However, the same kind of approach to the problem was found effective, and the whole investigation led to results of a far-reaching nature. The most important of these was the formulation of simple rules for the prevention of flutter, such, for example, as the correct mass-balancing of a control surface to prevent it from entering into a flutter of the whole wing, and the correct disposition of the axis of twist of the wing itself in relation to a certain axis defined by the aerodynamic derivatives to prevent bending-torsional flutter of the wing from occurring. Simple rules such as these, which the aircraft designer could easily apply, did much to eliminate the flutter danger, though it was and is still necessary to go back to more complete solutions of the equations when any new device, such as spring tabs on controls is proposed. It may here be mentioned that a special Flutter Sub-Committee of the Aeronautical Research Committee was appointed to advise on all matters relating to the flutter problem.

The study of flutter quickly led to investigations of cognate subjects, such as mechanical vibration, the elastic theories of bending and twisting, tail-plane buffeting and the reversal of aileron control at high speeds, in all of which Duncan played a conspicuous part. Matrices had been applied by Frazer to the solution of linear differential equations with variable coefficients and this led Duncan to an interest in matrix methods and to their application to problems of mechanical vibration. He found that the natural frequencies of dynamical systems could be conveniently found by operations with the matrices of the coefficients. In this work he was ably assisted by Collar, who later collaborated with Frazer and Duncan in writing the book *Elementary matrices*, a book which has been influential in demonstrating the value of matrices in classical mechanics, in the theory of differential equations and in numerical mathematics, and is still a standard work on the subject. This book has now been reprinted four times and has also been translated into Russian and Czech.

A special branch of flutter theory is concerned with the aerodynamic theory of the derivatives, that is, with the forces on an aerofoil or other body in unsteady motion. The foundations of such theory were laid by H. Wagner in Germany, but H. G. Glauert (later F.R.S.) in England, was the first to calculate correctly any of the derivatives of wing flutter. Duncan and Collar followed up his work and obtained the complete set of derivatives for a thin aerofoil in two-dimensional motion, covering oscillations of constant amplitude, exponential oscillations and non-oscillatory divergencies, and the results were published in *Reports and Memoranda* No. 1500. Theories of this kind have since been notably extended by Theodorsen in U.S.A., Miss H. M. Lyon, in this country, first extended the theory to three dimensions, and probably the most satisfactory three dimensional theory so far propounded is that of W. P. Jones, the present Superintendent of the Aerodynamics Division at the National Physical Laboratory. This is a good example of development resulting from an initial impulse largely due to Duncan.

Duncan's fondness for mechanical devices is well illustrated by an interesting apparatus which he had made, and which he called a 'flutter engine'. This consisted of a short rigid aerofoil held by a spindle in one end. The spindle was so mounted that the aerofoil could flap up and down and also rotate in pitch about the spindle as axis. These two motions were not, however, independent, but were geared together by means of toothed wheels so arranged that the phase relation of the two motions could be altered at will. The device was placed on the floor of a wind tunnel and a rod by means of which the phase angle could be adjusted, was led through the tunnel wall. Over a certain range of phase angle the model aerofoil would flutter violently, showing that in this condition it could extract energy from the air-stream: hence presumably Duncan's word 'engine'. When the phase was altered ranges could be found in which the model was completely stable. Duncan, needless to remark, showed that this was what would be expected from theory. Although Duncan spent most of his time at the National Physical Laboratory on investigations of flutter and allied subjects, he took a great interest in all the aerodynamic research which was going on there. Occasionally he had ideas in other fields which led to published reports. Thus we find him writing on a modification to the Chattock tilting manometer to eliminate the effects of temperature on its zero, and also some notes on the theory of anemometers. In 1930 he obtained the degree of D.Sc.(Eng.) from the University of London, submitting a theses on 'The wing flutter of biplanes', and supporting papers. He also, at the instigation of a few junior members of the staff, gave a series of talks on fluid mechanics, and this may have helped his inclination towards a subsequent academic career.

Duncan left the National Physical Laboratory in the summer of 1934 to take up the appointment of Wakefield Lecturer and Head of the Department of Aeronautics in University College, Hull. Lord Wakefield, in 1933, had made a gift to enable an aeronautics course to be established, in the first instance for an experimental period of three years. The course, which led to a Diploma in Aeronautics, was run jointly by the University College and the Municipal Technical College for the benefit of students who wished to take up responsible positions in the aircraft industry. At first the number of students was small, but Duncan soon built up and consolidated the new Department, and, largely because of his enthusiasm and personal qualities, the number of students steadily increased. He was responsible for the installation of a wind tunnel and apparatus for the microscopic examination of metals, and under his guidance a sound and modern library of aeronautical books was developed. When the University College established a Chair of Aeronautics in 1938, Duncan was appointed the first Wakefield Professor. His work at Hull brought him into close contact with the Blackburn Aircraft Company at Brough, some ten miles away. He acted, in effect, as a Consultant to the Company, and there was some interchange of students between Blackburn's and the College. In 1939 the second world war interrupted Duncan's academic life and he left Hull, nominally on leave, to assist in the war effort. It may be well to note here that the Department he had founded was carried on for some seven years by Professor G. C. Steward, of the Department of Mathematics, with the assistance of Dr S. Kirkby, who subsequently went to Cranfield. The number and quality of the students continued to improve. Professors Steward and Duncan had planned to institute a B.Sc. course of three years' duration after the war, but unfortunately this never materialized, though it might have had a good future, and was warmly supported by Duncan.

Immediately after the outbreak of war Duncan joined the staff of the Royal Aircraft Establishment, Farnborough, as head of the research section of the Armament Department, where his main responsibility was to encourage the application of scientific knowledge to the many problems arising in air armament. He is particularly remembered for his contributions to the aerodynamics and control of winged torpedoes and gliding bombs, a subject to which his wide knowledge of stability problems found ready application. In June 1941 he was asked to go to Exeter and take charge of the Air Defence Research Establishment in succession to G. T. R. Hill, of 'pterodactyl' fame. This organization had been formed at Farnborough in 1936 as the Air Defence Department, largely under the influence of Sir Henry Tizard, and when war broke out it was transferred to the Washington Singer Laboratories of the then University College of the South West to enable flight experiments to be carried out with greater freedom than was possible in those days at Farnborough. The main object of the Establishment was to investigate and develop unorthodox means of air defence with particular reference to the threat of massed attack by heavily-defended bombers. It also dealt later with means for defending airfields against low-flying attack. Duncan was able, with his sound common sense and wide experience, to keep unorthodoxy within reasonable bounds, and he spent a year at Exeter which was a great help to the workers there. At about this time, and very likely arising from the work of the Establishment, his mind was turned towards the problem of the stability of parachutes, and he made a valuable contribution to this subject. In particular, he was able to apply theory to the problem of the 'squidding' of parachutes. Under certain circumstances the open rim of a parachute may collapse inwards without the parachute becoming completely deflated. The parachute then falls in a configuration rather like a decanter, mouth downwards, with greatly reduced drag and consequent high speed. Duncan was able to analyze this motion and to suggest measures to prevent its occurrence. He returned to the Royal Aircraft Establishment in 1942 as head of the Flight and Airborne Section. Work was going on here in relation to the improvement of the handling characteristics of aircraft, the problem of the landing of naval aircraft, matters of rocket-assisted take-off, gliders and army assault problems in general. Towards the end of the period the question of near-sonic flight assumed serious importance and a new group was formed to deal with it. Duncan's great experience in aircraft dynamics enabled him to lead his strong team with energy and confidence. His own contributions were not by any means negligible but most of his time was necessarily spent in supervisory work to see that the operations of the Section were rightly timed and co-ordinated with all the other work at Farnborough. One who collaborated with him at this time compared him to a 'very large and seaworthy ship surrounded by a turbulent sea of young and comparatively inexperienced men', an apt description of the impression that Duncan's personality made on many of his collaborators. He added that 'in spite of the turbulence the ship surged forward, usually in the right direction'. Soon after the end of the war Duncan spent a few months in Germany. He went to the Luftfahrtforshungsanstalt at Volkenröde and to the aerodynamics laboratories at Göttingen, where his duties were to supervise the transfer of these facilities to Allied control, and in particular to see what apparatus and equipment could usefully be transferred to laboratories in this country. At the end of 1945 he came back to join the staff of the new College of Aeronautics, then in course of formation.

In early 1945, when it appeared that the war was drawing to a close, renewed attention had been given to the proposal, originally emanating from Sir Stafford Cripps, to establish a College for post-graduate education in aeronautical science. The Aeronautical Research Committee had previously discussed the formation of such a College, and given its approval, on the basis of a paper presented by Professor Duncan. When it was finally agreed that this should be done, a body of four men was appointed to visit promising localities where the College might be established and to report their findings. The four were Dr W. Abbot, H.M.I., Major G. P. Bulman, Professor Duncan and myself, representing broadly the outlook of the Ministry of Education, the Air Ministry, the Universities, and the Research Institutions respectively. After some few visits to different places, we decided that the best offer was half of the Air Force Station at Cranfield, in Bedfordshire, the other half to remain as the headquarters of the Empire Test Pilots' School, at least for a time. Steps were taken to acquire equipment and to find suitable members of the College staff with a view to starting the necessary work on modifying the existing buildings and installing apparatus at the beginning of 1946, and hoping, as proved to be the case, to be ready for the first student intake in October of that year. Duncan had the distinction of being the first member of the College staff to be appointed, the date being 1 December 1945, while it was not until 1 January 1946 that the next two appointments were made, those of myself as Principal of the College, and V. F. Knight as Registrar.

Duncan made good use of his early appointment, and by the time we began to get together in the new year he had sketched out what he considered to be the right plan for the teaching in the College, a plan which was adopted and formed the basis of the instruction given for some years. There were only three Departments in the College at its inception: Aerodynamics, Aircraft Design and Aircraft Propulsion. Duncan was the Professor of Aerodynamics, and the success of his teaching became apparent in later years when many of his students reached high positions in the aircraft industry and in research. He became Deputy Principal in 1949. In addition to professorial duties in his Department he took a great interest in the College Library and did much to help the Librarian build up what soon became one of the best aeronautical libraries in the country, which did not confine itself to aeronautics but had a good representative collection of books on literature and art. Duncan firmly believed in encouraging students to read widely, and not to get into the rut of a single scientific specialization. He also had much to do with the establishment of the series of *College of Aeronautics Reports*, on researches carried out both by staff and students, which are now well known in aeronautical circles and run well into three figures in number. To Duncan goes the credit of writing the first of these *Reports*, as well as several later ones. He and I negotiated with the Cambridge University Press with a view to the publication of a series of text books of an advanced nature to be written by members of the College staff. This was originally entitled *The College of Aeronautics Aeronautical Series*, but after discussion with the Press it was felt that this title was too limited, and that authors outside the College should be encouraged to help. The final books were accordingly entitled the *Cambridge Aeronautical Series* and Duncan contributed the first book on *The principles of the control and stability of aeroplanes*, published in 1952, since when other volumes have appeared. Duncan conceived the idea of giving each new student entry a lecture which he called 'Telling the world what you have done'! This was a disquisition on the art of writing scientific reports. Coming, as it did, from an undoubted master of the art, it could hardly fail to be of great use to the students, and to impress upon them the importance of lucid exposition when later on they came to write reports themselves.

Most of the College staff, and especially the senior members, lived on the spot, in houses originally designed for the accommodation of Air Force Officers and men. Duncan, however, elected to live in Bedford, eleven miles away, mainly on account of the education of his daughters, and so he was not able to take part so freely as others in the social life of the College, which formed a very important feature of its corporate life. Nevertheless, he did come back often in the evenings to join in many of the social activities which went on, though he was not one of those who revel in such things.

Duncan was elected a Fellow of the Royal Society in 1947. In 1950 he left the College to become the first Machan Professor of Aeronautics and Fluid Mechanics in the University of Glasgow, so returning to the town of his birth, and this appointment he held until his death. Thus, for the third time in his life, he started to build up a new school from the beginning. This he did with his usual thoroughness and wisdom despite the fact that the high reputation he had now established in the engineering world inevitably led to many commitments external to the University. He had soon established himself firmly in the affections of his colleagues and his students, and the Department he created and nurtured is left as a worthy memorial to his labours. During the early period of his tenure of the Chair he was honoured by the Sovereign by appointment as C.B.E., and at the very end he received the highest award of the Royal Aeronautical Society, election as an Honorary Fellow. He was able to spend much time on personal research once more, and published nearly twenty papers, mainly on his favourite subjects of kinematics, stability and matrix methods.

In 1957 Duncan was appointed Chairman of the Aeronautical Research Council, a position which he held until the end of his life, and discharged with conspicuous success. He had been associated with the work of the Council since 1936 as a member of various Committees and Sub-Committees and a member of the Council itself as early as 1939. It would be tedious to enumerate in detail all the valuable service he rendered in this way. Suffice it to say that he had been Chairman of two of the three Standing Committees, those dealing with Aerodynamics and Mechanics, as well as of four other bodies in the Council's organization. His membership at various times extended to as many as fifteen Committees and Sub-Committees of the Council, and of those which were concerned with his main interests, the Oscillation Sub-Committee and the Stability and Control Sub-Committee, he was a member continuously for well over twenty years. That he could undertake all this advisory work in addition to his academic duties is a tribute in itself to his energy and to his determination to do all in his power to further the cause of aeronautical science.

Such is a brief account of Duncan's life and work. It has previously been said that his nature was reserved; indeed he records that as a child he was exceedingly shy. Rubbing shoulders with many people removed the shyness, but left him with a good deal of the reserve, though he had a lively temper beneath the surface and this would flare up in no uncertain manner on rare occasions. In his work and writings he was meticulous almost to a fault, and gave long and careful consideration to any investigation before committing himself to paper. He always had clear and definite views about men and things and did not hesitate to express them emphatically, when necessary. He had an intense conviction that narrowness in science was a very great fault, and held that breadth of interest, even if not always accompanied by full understanding and knowledge, was a most desirable thing. The history of science and the biography of scientists seemed very important to him as a necessary part of a scientist's knowledge, and he had a lively appreciation of the supreme scientific quality of many old pieces of work. As far as I have been able to discover he had no absorbing hobbies, except for his life-long interest in botany and the beauty of nature. Apart from home and family his great preoccupation was with his scientific work, to which he devoted immense painstaking care and by which he has left a legacy which will be of great value to the engineering world for many years to come.

I wish to acknowledge the valuable help that has been given to me by a considerable number of Duncan's collaborators and friends, both in the Universities with which he was associated and the Government Establishments in which he worked. Special thanks are due to Mrs Duncan, who helped enormously by sending me all the relevant papers she found amongst her husband's effects and who also took the trouble to write to Duncan's sister and to several other persons to cull additional information about the earlier years of his life.

Ernest F. Relf

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T. R. Elliott

THOMAS RENTON ELLIOTT

1877-1961

1. *Introductory*

T. R. Elliott, born in 1877, was elected to the Fellowship of The Royal Society in 1913, before he was 36 years old; and his election, thus rather early in his career, was certainly made in recognition of the quite unusual distinction and brilliant promise of the experimental researches in physiology, which he had carried out at Cambridge by 1906, and of which he had published the results, with his conclusions, in several remarkable papers, between 1904 and 1907. College and University post-graduate studentships had enabled him thus to stay in Cambridge for the five years following his graduation in 1901, and there to concentrate his activities and interests on these experimental studies.

And then, in 1906, at the age of 29, he left Cambridge to enter as a student at University College Hospital, London, with the immediate purpose of completing his clinical curriculum and his qualifying graduation in medicine. Among those who had followed and admired the progress of what he had already then achieved, there were probably many to whom it would have seemed natural to expect that the years of clinical study and qualification would prove for him, as they had done for others, to be an interlude, the experience gained in which would strengthen his equipment and his claim for advancement in an academic career, most obviously perhaps in physiology, but, in any case, in one or another of the experimental disciplines which had by then already become recognized as contributory to the scientific background of medicine. And, during the following decades, there was, indeed, a good deal of speculation among such observers, concerning factors which might possibly have been concerned in the obvious reduction, though still for some years the incomplete diversion of Elliott's activities, from what had seemed to be the line of his most natural interests and aptitudes, and therefore that of the greatest promise for his continued contribution to the advancement of medical knowledge. To those, indeed, who were not more intimately in his confidence, during the years which had followed his transfer to University College Hospital, up to and including those of the first World War, it might well have seemed that the influence of special circumstances must be assumed, in explanation of the fact that personal participation in experimental research, either in physiology or, indeed, in any part of the medical field, was not, thenceforward, to occupy the predominant position in Elliott's interests and activities which it had formerly held. It may be, indeed, that some among Elliott's friends and admirers are still concerned with such speculations. To me, however, they seem to have been misdirected. For, in talks on a footing of some intimacy which I had with him during those earlier years of his in London, he had made clear to me his own intention to abandon any idea of resuming his full activity in experimental research, of the kind in which he had been engaged in Cambridge. He had enjoyed his experience of this with a keen relish, and was still ready to recall the thrills of excitement and intellectual satisfaction which it had given him, and, as opportunity served, to add some additional points to his earlier findings. But he had, in effect, decided already to regard that experience as an episode, having come to feel that, in the future, he would probably be able to give more and more of his best service to the advancement of medical science—an ideal to which his devotion remained deep and consistent—as a promoter and organizer of the direct opportunities of experimental research for others than himself, and, in so doing, to encourage the extension of its methods of precision and its standards of evidential significance to a wider field of medical studies, and especially to the clinical disciplines. It would be unrealistic, of course, to suggest that Elliott was to no extent influenced by the general movement of ideas at that time towards a more orderly and scientific advancement of knowledge in all departments of medicine, or by the new associations which he encountered and formed, when he moved from the detached and academic atmosphere of the Cambridge laboratory to the practical responsibilities of hospital wards. I am only concerned to record my own impression, strengthened by talks which I had with him at the time, that Elliott was not more but, on the contrary, less liable than most of us, to allow extraneous influences of any kind to affect his own judgement in the planning of his career. It seemed clear to me that he had come to recognize qualities of ability and character in himself, which, thenceforward, were likely to find their proper satisfaction, and to make their best contribution to the common cause, in organizing and administrative responsibilities for the promotion of research, rather than in a further personal conduct of it. I believe that it was wholly in accordance with Elliott's character, thus firmly to make up his own mind, and then to wait for the opportunities which would enable him to make the decision effective.

It is clear, in any case, that an adequate appreciation of Elliott's scientific career must recognize the difference in kind between his contributions to the advancement of knowledge in the special field of medicine at these two different and unequal periods.

2. *Parentage and Education*

Thomas Renton Elliott was born on 11 October 1877, at Springfield, Willington, County Durham, the son of Archibald William Elliott (1845-1928) by his wife Anne (1849-1929), the daughter of Thomas Renton, of Otley, Yorkshire, after whom, as will be seen, our T. R. Elliott was named. The father, A. W. Elliott, had a general, retail business in Willington, and is remembered as a kindly man, of scholarly instincts and conversation, with a special interest in music. Elliott was inclined to trace this musical faculty to his paternal grandmother, born Margaret Armstrong (1813-1908), who, as will be seen, lived to a great age. He also spoke, with a grateful pride, of the strong character and ability of his paternal grandfather, Peter Elliot, who had started his career as a joiner and wheelwright—a craft which, in its essence, he had apparently inherited through more than one generation—but had then qualified himself by his own effort and ability to become the Parish Schoolmaster at Lamesley, County Durham. Peter Elliot had two sons senior to T. R. Elliott's father, one of whom has a special interest for us, as giving evidence for a trait of special and distinguished scientific ability, in that generation of the family. This was the eldest son, John (1839-1908), our Elliott's paternal uncle, who went to St John's College, Cambridge, presumably with a scholarship in mathematics, became Second Wrangler and Smith's Prizeman, and a Fellow of his College; he then entered the Indian Civil Service, to become Professor of Mathematics at Roorkee and Allahabad, and of Physical Science at Calcutta. He did important service in organizing meteorological work in India and elsewhere, became Director-General of Indian Observatories, was made a K.C.I.E. and a member of the Viceroy's Council, and was elected F.R.S. in 1895—the year before his nephew, T. R. Elliott, entered Trinity College, Cambridge. Some might find the tracing of these relationships a little complicated by the varied spelling of the family name. Peter Elliot still spelt his name in the usual Scottish manner, with a single 't'. Our Elliott, following, his father used the Anglicized form, with two 'l's' and two 't's'. Sir John Eliot, on the other hand, used what, I am told, is properly the Cornish spelling, with one 'l' and one 't'; having apparently been led to adopt this, in order to avoid confusion with another Elliot, serving with him on the Viceroy's Council in India.

Till he was eight years old, Elliott was taught at home by his mother. He was then sent, for four years, to a preparatory school, 'The Mount' at Northallerton, in Yorkshire. Then, in 1899, he entered Durham School as a King's Scholar, becoming a member of the Second Master's house. By 1894, when he was 17, he had become the Head Boy, for his two final years at the School. He became a member of the Rugby Football XV in 1895, of the Cricket XI in 1895-6, and also won the Challenge Cup at Fives in 1895. Thus, we have the picture of a boy of good ability, character and physique, but with no record, so far, to suggest intellectual ability in a special direction; and there is naturally nothing to suggest an opportunity for scientific training, outside the conventional curriculum of a school of that type at that period. He certainly retained proud and affectionate memories of the Durham School—an ancient Benedectine foundation, 'refounded', or not despoiled, by Henry VIII, when he dealt with the parent monastery. It had remained a near neighbour of the great Cathedral, with its Norman 'Galilee', or Lady Chapel, containing the tomb of the Venerable Bede. In this Chapel the School, in Elliott's time, held a daily service, as well as those on Sunday; and Elliott was certainly susceptible to, and retentive of, the impressions created there and elsewhere by such great historical associations.

He left the Durham School in 1896, when he was nearly 19 years of age, being awarded a Leaving Exhibition. Having dealt with his Cambridge 'Little-go' and sat for examination at Trinity College, he was awarded a Sub-sizarship in Classics, with the help of which he was able to enter the College. With medical qualification already in view, the subjects chosen, under advice, for his Tripos would include some, such as physiology and human anatomy, which would have been entirely new to him at entry. At that date, however, in physiology especially, there was still so comparatively small a range of knowledge to be acquired, that it would be much more readily covered from the start, in a three-year course of study, than it can be now, at the present rate of its expansion. The record suggests, in fact, that Elliott required four, instead of the normal three years, before he presented himself for Part I of the Natural Sciences Tripos, carrying the B.A. Honours Degree. This, however, was the result of a misfortune without which, indeed, such a postponement would not have been possible. Before Elliott could sit for examination in his third year, 1899, he fell victim to appendicitis, and then to typhoid fever—the latter not being regarded in those days as such an anomaly, or such a hygienic scandal, as it would be today. He was accordingly obliged to obtain permission to 'degrade' for a year, so that he could take his Tripos, Part I, in 1900, his fourth year. He was then duly placed in the First Class; and Trinity College, in the same year, awarded him a full Sizarship—an emolument equivalent in money value to a Major Scholarship, but awarded with reference to private resources, and not carrying the status of a Scholarship in the College. And then, in the following year, 1901, he took Part II of the Natural Sciences Tripos, with physiology as his special subject, and again with First Class Honours; and Trinity, in this year again, made him a Major Scholar of the College.

So far, then, the third-year misfortune might be thought to have involved nothing more serious than a passing inconvenience and, perhaps, an extra strain on resources. One could imagine a case, indeed, in which the balance of extra time available for preparation might have had some compensatory advantage for a student. For Elliott, however, a misunderstanding of the effect of this delay, in a different connexion, was to involve a shattering disappointment. It concerned his application for a Trinity College Fellowship, for which, by that time, he had come to be recognized as a candidate with unusually strong claims. If he had been able to take the examination for Part II of the Tripos in the normal fourth year, there would have been two years then ahead of him, in either of which, or in both if he failed at a first attempt, he could have submitted a thesis for consideration. He had supposed that the University's leave to postpone his Tripos on account of illness would carry with it a corresponding postponement, by the College, of the two years available for Fellowship applications; and it appears that the distinguished philosopher who was his College tutor had even confirmed him in this assumption. Wishing, then, to have as long a time as possible for the development of the new research on which his thesis would be based, he had decided to wait till his second post-Tripos year, before submitting an application. And then it was discovered, all too late, that he had been wrongly advised, and that he had, in fact, missed his only opportunity, under the College Statutes, for presenting his claim; so that he was never able to be even a candidate for a Trinity Fellowship, and nothing less than a special Act of Parliament could have made him so. Later, in 1908, when Elliott had already been for two years at University College Hospital, Clare College offered him a Fellowship, which he gladly accepted, and was able therewith to renew and to retain contacts with friends and affairs at Cambridge for some years; and then, many years later, in 1947, he learned in retirement, and to his great joy and satisfaction, that Trinity College had elected him to an Honorary Fellowship.

I myself had entered Trinity College two years before Elliott; and in that large community such a difference in seniority, in the absence of special circumstances, meant but few opportunities of close personal contact. During the four years by which our periods at Trinity overlapped, I had, accordingly, only just come to know him, and mainly by sight and reputation, as a student whose ability was making a strong impression on his teachers and contemporaries, already marking him as likely to become a strong recruit, in due course, to the ranks of the research-workers in physiology. Otherwise I heard mention of him, by other friends of those days, as playing Rugby football, and as a keen member and N.C.O. of the University Rifle Volunteers. In 1900 I left Cambridge, where I had for the last two years been sharing the Coutts-Trotter Studentship—a postgraduate studentship created by a former Vice-Master of Trinity College, for research in physics or physiology. I was glad to hear later that Elliott had been elected to this Studentship; but I practically lost sight of him then for a few years, till the researches in which he was engaged began to bring him to clearer notice at meetings of the Physiological Society.

3. *Physiological researches at Cambridge*

The Department of Physiology at Cambridge, when Elliott entered it as Coutts-Trotter Student in 1900, had already for some years been a widely famous centre of research, as it has continued to be. The Professor, Sir Michael Foster, had never been prominent as a research worker, but had a remarkable faculty for encouraging research in others and for criticizing its results. His pupil and friend Dr Walter H. Gaskell had already by then left his pioneer work on what he called the 'involuntary' or 'visceral' nervous system, to be continued and elaborated by Foster's other distinguished pupil, and his eventual successor in the Chair, J. N. Langley. Langley had introduced and established an alternative nomenclature, in which, for example, the part of the system which Gaskell had referred to as 'the cranio-sacral division of the visceral nervous-system' became 'the parasympathetic division of the autonomic system'. These two great physiologists were strongly contrasted in temperament and attitude. Gaskell, a free-lance worker at Cambridge, delighted in theories, and his short course of advanced lectures gave the hearers an inspiring vision of research, and, of course, of physiological research in particular, as an exciting intellectual adventure. 'So I thought I could test this idea by an experiment', he would say, and a description of this would follow; and then, 'to my surprise and delight', he would exclaim—the result had, of course, given clear support to his speculation. Anybody working then, like Elliott, in that laboratory, and in a field in which Gaskell had been active, would be eager to keep him in touch with what was being observed and interpreted, with a full assurance of his wise criticism, keen interest and warm encouragement. Langley, by contrast, was temperamentally sceptical and critical of theories, claiming to be interested only in facts, with a rather dry succession of which his lectures, indeed, were often apt to be chiefly concerned. 'Make accurate observations and get the facts', he would say; 'if you do that the theory ought to make itself.' Such advocacy of a purely inductive method had its value, of course, as a discipline for a beginner who exhibited an excessive eagerness to expound his speculations; but it was apt to be discouraging to genuine enthusiasm. In addition to these seniors, the Department of Physiology, small and old-fashioned as its accommodation was then, managed to find room of a sort for a number of other research workers of different ranks and different claims to distinction. W. B. Hardy had already been led from his direct study of living organisms to more fundamental, physico-chemical researches on the colloidal state. H. K. Anderson had for years been collaborating with Langley, and his fine technical skill and enthusiastic industry were making a major contribution to the classical series of researches, in which they had been unravelling the complex of the autonomic system. Of men nearer to Elliott in standing, Joseph Barcroft, his senior by four years, was already engaged in what was to be his life's work on the respiratory functions of the blood; Walter Fletcher, with whom Elliott was later to collaborate most intimately, in London, was still busy then with physiological experiments, already a Fellow, and soon to be a College tutor at Trinity; somewhat later, Keith Lucas was beginning his pioneer studies of the nature of the conducted excitation in muscle and nerve; while Frederick Gowland Hopkins, later to receive a separate Chair and Department of Biochemistry, was then giving his lectures and conducting his still relatively small and intimate practical class, in such close quarters as the old Department of Physiology could afford. Certainly, then, Elliott did not lack collateral stimulus from the buoyant spirit of scientific enterprise in the Department as a whole; and I can remember his commenting on this to me, with keen appreciation and pride.

As the holder of Trinity's Coutts Trotter Studentship, Elliott had Langley as Director of his researches; and it was natural, accordingly, and entirely congenial to the special line of interest which Elliott seems already to have formed for himself, that Langley should encourage him to try his hand at some special problem connected with the visceral or autonomic nervous system, starting perhaps with the abdominal viscera, and with their characteristically double and in many cases antagonistic innervation. When once he had been given the opportunity, however, and had found, under advice, a suitable subject for research, Elliott was evidently in no hurry to publish, until his experiments had given him a substantial yield of new knowledge. His researches apparently began in 1901, when he still held a Trinity Scholarship and with it a Jeston Exhibition; and his tenure of the Coutts Trotter Studentship began in 1902. His first publications, however, did not appear till 1904, in Volume 31 of the *Journal of Physiology*. His first full paper, beginning on p. 157 of that volume, was on 'The innervation of the ileocolic sphincter in the cat'. As Elliott pointed out, the term 'ileocaecal valve', introduced by Caspar Bauhin in 1579 to describe the special, valvular conformation of that junction between the small and the large intestines of man, though it had been in general use in reference also to the corresponding transitional structure in the carnivora and other laboratory subjects, had no proper application in these other species. The junction of the ileum with the colon (not with the caecum) in the cat, dog, or rabbit, was guarded, not by a valvular, slit-like opening of the terminal ileum, as in man, but by a muscular sphincter. The main point of the paper was to demonstrate that the fibres of this sphincter are thrown into a contractile spasm when their nerve supply from the true sympathetic system is stimulated, whereas the tone and natural rhythms of the muscular walls of the ileum above this sphincter, and of the colon below it, are deeply inhibited by stimulation of the same nerves. On the other hand, he was unable to detect any but inconstant and weak effects on this sphincter from stimulation of the pelvic visceral nerve-supply, which caused so intense a contraction of the colon below it. The description of these results and the consideration of their significance were clear and vivid, and the evidence given, in this first full publication by a new investigator, of high degrees of manipulative skill, of care for detail, and of ingenuity in the devising and use of such simple but effective means of mechanical recording as were then available, was singularly impressive. This paper was further noteworthy as containing the first case recorded in detail by Elliott, of the close correspondence between the effects on the structures concerned—in this case on the muscular walls of the ileum and the colon and on the ileocolic sphincter—of the effects, on the one hand, of stimulating the sympathetic nerves and, on the other hand, of injecting the then recently available adrenaline into the circulation, or of applying it locally to the different structures.

We shall see that the much wider correspondence to be observed between the effects of sympathetic nerve impulses and those of adrenaline, over the whole range of the involuntary muscular and glandular organs, in all the different animal species in which he was able to make the comparison, together with the physiological significance of this remarkable correspondence, was to provide Elliott with a kind of central theme for his researches in this post-graduate period at Cambridge. No special reference to this generalization is given, however, in this first paper on the ileocolic sphincter, or in another and longer paper, published in this same year and volume (*J. Physiol.* **31,** 272, 1904) by Elliott jointly with Dr Barclay-Smith—already then a senior teacher of human anatomy in the University—in which they gave a further description of the structure, movements and innervation of the large intestine—colon, caecum and rectum—in various animal species. In this paper also there is mention of the correspondence between the effects, or lack of effects, of stimulating the sympathetic nerve-supply and those of adrenaline on the corresponding parts of this system; and, further, of the absence of any such correspondence, but rather an antagonism, between the effects of adrenaline and those of stimulating the pelvic visceral nerve-supply. In neither of these earliest papers, however, is there any special discussion of the significance of this correspondence in the one case, or of its absence in the other.

There is to be found, however, in this same volume of the *Journal of Physiology*, as an abstract of a communication which he made to a meeting of the Physiological Society on 21 May 1904—only a few weeks after the appearance of his first paper on the ileocolic sphincter—a preliminary account of the much wider survey of this resemblance, in which, by then, Elliott must already have become eagerly engaged (*J. Physiol.* **31,** Proc. Physiol. Soc. p. xx). For he was ready even then to put forward, and to include in the published abstract, the most explicit statement which he was ever to publish concerning his own idea of the possible meaning of this very striking correspondence, between the effects of what had appeared to be two such different orders of stimulation. At that still early stage of his experience in research he had seen already that this remarkable similarity of effects might be due, in fact, to a community of cause; that sympathetic nerve impulses, on reaching the endings of the nerve fibres, might there liberate small charges of adrenaline, to transmit their effects to the contiguous effector cells. This conception, of the transmission of the effects of a nervous impulse across a neuro-effector or synaptic junction by the release there of a specific chemical transmitter-substance, has by now been so much more widely applied, and has, indeed, become such a commonplace of discussion among physiologists, that there may be some who have forgotten, and even some, perhaps, who have never known, when and with whom it originated. It may accordingly be appropriate, in commemorating Elliott and his work, to reproduce the passage of this preliminary note, in which, with a sufficiently tentative modesty, but none the less explicitly, he suggested such a mechanism to explain the adrenaline-sympathetic parallelism, the full extent of which he was then still exploring.

'But since', he wrote, 'adrenalin does not evoke any reaction from muscle that has at no time been innervated by the sympathetic, the point at which the stimulus of the chemical excitant is received, and transformed into what may cause the change of tension in the muscle fibre, is perhaps a mechanism developed out of the muscle cell in response to its union with the synapsing sympathetic fibre, the function of which is to receive and transform the nervous impulse. Adrenalin might then be the chemical stimulant liberated on each occasion when the impulse arrives at the pheriphery.'

And there is a further abstract, from a later meeting, to be found in the same volume (*J. Physiol.* **31,** Proc. Physiol. Soc. p. lix), dealing with 'The reaction of the ferret's bladder to adrenalin', in which Elliott describes another example of this correspondence in effects; sympathetic nerve stimulation and adrenaline both inhibit the tone of the cat's bladder and increase that of the ferret's, while neither has any effect on that of the dog or the rabbit. These four publications in 1904 were, indeed, a noteworthy output for one who might well have been regarded as still in his early apprenticeship to research. In the following year, however, it became clear that they had been, in effect, preparatory to the appearance of a paper of major scope and importance, published in the next volume of the same journal (*J. Physiol.* **32,** 401, 1905), still dealing with 'The action of adrenalin', and with its remarkable similarity to that of impulses in the sympathetic nerves, but now in a most comprehensive manner, and with a great volume of varied evidence, much of it newly available from Elliott's own experiments.

This paper extended to 68 pages, of a journal which contributors had come to regard as being strictly—some would have said almost fiercely—edited by Professor Langley. Reading it again today, I find myself still captured by the vivid interest of the narrative, with new experimental evidence presented on nearly every page, or existing evidence in the light of new comparisons, and all with a scrupulous regard for accuracy of details. Discussion was not confined to the paper's own immediate subject. Here, for example, is a passage to which Elliott himself gave a special emphasis by a double spacing of the type, and which, in this connexion, seems today almost uncannily prophetic of what is now known about the function of a common chemical transmitter at the parasympathetic peripheral endings, the synapses in autonomic ganglia and the voluntary neuromuscular junctions. This is what Elliott wrote in 1905 in this paper mainly concerned with adrenaline:

'The specific reaction to adrenalin(e) marks the deep distinction between the myoneural junctions of the sympathetic nerves on the one hand, and of the cranial or sacral autonomic on the other together with all the preganglionic "synapses" which are rather related biochemically to the junctions of the skeletal nerves with the striped muscles.'

And he was further to emphasize his suggestion, that these nerve junctions are thus 'related biochemically', in a communication to the Physiological Society later in the same year (*J. Physiol.* **33,** Proc. Physiol. Soc. p. xxix, 1905) on the effects of denervating the cat's bladder. He was basing his speculations also, of course, largely on evidence about the actions of other substances, including curare and nicotine, obtained in earlier investigations, from those of Claude Bernard to those of Langley, his own Research Director; but he seems to have gone much further than anybody before him in his instinctive recognition of a 'biochemical' connexion, for the physiological meaning of which there was then to be no further clear hint, until the effects of acetylcholine at all these junctions came to notice nearly 10 years later.

It would not be suitable here to attempt any systematic review of the details of this remarkable paper. It still appears to me to deserve recognition as one of the physiological classics of its period. I myself had been looking forward with a special interest to its appearance, partly because, in experiments of my own, I had chanced to encounter the highly selective effect of a certain constituent of ergot, which abolished only the augmentor effects of sympathetic nerves and the corresponding augmentor effects of adrenaline; and I had therewith provided some further examples for Elliott's general thesis. These results of mine had, in turn, excited his keen interest, and, as mentioned in his paper, he had eagerly agreed to come to London for a week, to work on them with me in my laboratory. I still recall that week, during which we found together new points of significance during every day, and spent each evening until the small hours in discussion of their meaning. For me it remained one of the 'high spots' in my many years of experience in research. Elliott was my junior by more than two years; but in this field, at least, it seemed natural for him to lead, and for me to follow. And it was quite clear to me at that time, when he was already deeply engaged with the preparation of his great paper of 1905, that there had been no weakening of his own belief in the specific transmitter-function of adrenaline—the then highly original, but, as developments many years later were to show, the essentially true conception, which he had already grasped, and had put forward in his preliminary note. And yet, when the full paper was published, the one point in which it disappointed my expectations, was in its failure to make more than a passing and indifferent reference to this brilliant suggestion, which had seemed to me, and seems to me still, to have been the most important and far-reaching result of Elliott's researches. In this full paper, he made an elaborate and convincing case, supported by a most impressive array of additional evidence, for the earlier conclusion that adrenaline acts, not on the endings of nerve fibres, but on some constituent of the effector cells, present only in those which have been innervated by sympathetic fibres, but surviving degeneration of these fibres, and with its sensitiveness to adrenaline even enhanced thereby. Much of this, however, gave only confirmation and clearer definition to conclusions which earlier investigators —Lewandowsky, Langley, Brodie and Dixon—had already put forward in essence, though with variations of detail and terminology, and with much less complete and convincing foundations of evidence. When he came, however, to discuss the physiological significance of these findings, he might have been supposed to have come, already, to doubt the real significance of his own most illuminating suggestion of a transmitter function for adrenaline.

For this now appeared only as one, and the last to be mentioned, of a number of possible functions which various authorities had suggested for this substance, and of which none of the others seemed to be even very plausible, or to have any special relevance to the characteristic mimetic action. Here is Elliott's concluding reference to his own suggestion of the transmitter function, including it in an open verdict, as it were, with all the others which he had thus listed:

'or the conjecture that it (i.e. adrenaline) is concerned in the transference of a sympathetic nerve impulse, and stored to such an end in the neighbourhood of the myoneural junction.

'The evidence does not conclusively disprove any of these.'

'Not conclusively disprove'! One might almost suppose that the transmitter theory, which has since been so well established, had then somehow got into bad company, and had only escaped conviction and sentence for want of sufficient evidence.

What, then, can have produced such an apparent change in Elliott's attitude to his own penetrating suggestion? There can be no doubt, I think, that he had been deeply discouraged by discovering that it had aroused so little serious interest among physiologists in general, and that its reception by Professor Langley, in particular, had been distinctly unfavourable. It must be remembered, in that connexion, that Langley himself had earlier worked on these phenomena with a crude suprarenal extract, before adrenaline became available (*J. Physiol.* **27**, 237, 1901). Extending Lewandowsky's observations, and confirming the fact that, in the cases tested, the specific actions of the extract persisted after degeneration of the corresponding sympathetic nerve-supply, Langley had found it 'hardly possible to avoid the conclusion that in these cases the extract acts directly on the unstriated muscle'; and then, further, that 'In such case(s) the difference in action on autonomic tissues must depend on their intrinsic differences; and this takes out of our reach any immediate hope of explanation why the action in the several cases should correspond so closely with that caused by stimulation of the sympathetic nerves'. Langley, as already mentioned, was always inclined to disapprove of speculative theories, on principle; and he could hardly have been expected, perhaps, to make an exception in favour of one which had been proposed by a research student working under his direction, and which offered an explanation so simple and complete, in a case for which he had himself concluded, only a few years earlier, that 'any immediate hope' of such was 'out of our reach'. For whatever reason, Langley was apparently unwilling even to discuss Elliott's transmitter suggestion; for he made no mention of it in a long and argumentative paper which he published towards the end of the same year (*J. Physiol.* **33**, 374, 1905). In this he devoted much discussion to the respective merits of the term 'myoneural junctions', which Elliott, following Brodie and Dixon, had used to describe the point of action of adrenaline, and the term 'receptive substances', which Langley now proposed to use in the same kind of connexion. The difference, though argued at length, might have seemed little more than verbal; and, whatever its author's intention, and although he made flattering reference in it to the ability of Elliott's paper, it cannot be doubted, I think, that this publication by Langley, carrying his very distinguished authority, was largely responsible for diverting attention from what should have been recognized as by far the most important result for physiological theory, of any of the work on adrenaline at that period.

There has been some discussion, in recent years, concerning the origin of the more general theory of a chemical transmisison of effects at neuro-effector and synaptic junctions. Mention has even been made, in that connexion, of a primitive speculation by Dubois-Reymond, who had considered whether a nerve impulse might possibly liberate lactic acid, to excite a voluntary muscle to contraction; while my distinguished old friend, Professor Otto Loewi, who, many years later, was himself to give the first direct experimental demonstration of a chemical transmission—that of effects from vagus and sympathetic nerve endings to the frog's heart—has recently recalled (*Perspectives in Biology & Medicine*, **4,** 17, 1960) a talk which he had in 1903 with the late Sir Walter Fletcher, then visiting Marburg, during which the idea had occurred to Loewi that such neuromimetic drugs as muscarine might act as transmitters from nerve terminals. Dr Loewi had then, in fact, not long returned from England, where he had spent a part of his visit in Cambridge, and had there met the young Elliott, of whose work and outlook he had then spoken to me with a generous enthusiasm. It may well be, I think, that ideas of the kind in question were at that time to some extent 'in the air', and especially in that of Cambridge. To me, however, it has always seemed quite clear, that the earliest public and specific suggestion of a transmission from nerve-endings of excitatory or inhibitory stimuli, by the liberation there of a chemical transmitter natural to the animal body, was that made and published by Elliott in 1904. And, for my own part, I am convinced that any contribution which I may have been able to make in later years to the general development of that theme, owed its initial stimulus, and a continuing inspiration, to Elliott's published researches and ideas, and to my personal contacts with him in those early days.

Papers which Elliott published in 1906, with I. L. Tuckett on the morphology of the suprarenal gland, and, with H. E. Durham, in disproof of an antigenic property which others had attributed to adrenaline, were of relatively minor importance. In 1907, however, he published the second and last of his great physiological papers, on 'The innervation of the bladder and urethra', (*J. Physiol.* **35,** 367, 1907). Although this did not appear until the year after he had left Cambridge, it was almost entirely concerned with experiments which he had carried out there, with the support of the George Henry Lewes Studentship, awarded to him when he had finished his tenure of the Coutts Trotter Studentship. This paper was of even greater volume than the earlier one on adrenaline, running to 80 pages of the *Journal*; but, again, most of it was so filled with vivid and concise descriptions of ably designed and skilfully conducted experiments, and with enlightening, comparative discussions of the significance of their results, that I do not think that a qualified reader could have recommended any proper omission or curtailment. Elliott had started his widely ranging studies of the double innervation of the bladder and urethra, with those of the cat; and he had extended them to those of the dog, the rabbit, the ferret and the monkey (*Macacus*), and, in less full detail, to those of the Indian mongoose and civet cat, the pig and the goat—nine animal species in all, it will be seen; and, in hospital, he had later used an opportunity offered by an operation, to observe the local action of adrenaline on the wall of a human bladder, and had concluded that it gave no noteworthy response to that agent, so that, by inference, it would not be significantly controlled by a sympathetic (hypogastric) innervation—resembling in that respect the bladders of the dog and the rabbit, and differing from those of the cat and the ferret.

Like the earlier paper on the action of adrenaline, this one on the bladder, completing the record of Elliott's researches at Cambridge, represented so wide and so enterprising an attack on its subject, that such knowledge of this as could be experimentally obtained, with the methods and resources then available, might well have appeared to have been exhausted. It was, again, an astonishing achievement, both in the range shown by its record of what had been accomplished, and in the mature authority of its outlook. In the section devoted to a more general discussion of the significance of his findings, Elliott recurred to and further elaborated his suggestion, based largely on 'biochemical' or pharmacological similarities, that the nerve cells of autonomic ganglia may be regarded as morphological equivalents of the motor end-plates of voluntary muscle fibres; a suggestion of which the true significance was to be emphasized by much later discoveries. It is noteworthy that he made no direct reference in this paper to his chemical-transmission theory; but he took this, his last natural opportunity, for a courteous but stringent criticism of Langley's conception of 'receptive substances', as cell-constituents with specific affinities for particular chemical stimulants, arising independently of innervation, but becoming correspondingly responsive also, for no imaginable reason, to the impulses in a particular system of nerves, and to those in no others.

Elliott, however, made no further reference here to his own conception of a chemical transmitter function, which had offered so simple and direct an explanation for the otherwise mysterious neuromimetic phenomenon. And we shall see that, in a lecture given seven years later, just before the first World War, he was ready to advance what he by then, most surprisingly, had come to regard as a sufficient reason for no longer entertaining this attractive theory. If, when he first proposed it, it had received the authoritative encouragement which it clearly deserved, it is just conceivable that Elliott might have laid his plans with a view to a main career in experimental physiology. To me, however, it seems unlikely that he would have done so in any case. When this paper on the innervation of the bladder was published, he had already begun to acquire interests of a different kind, with his new, clinical contacts; and it was not long before he had begun to foresee that there would be scope for the development and application of other aspects of his strong character and unusual ability, and that his prior duty to medicine as a science might well lie in these other directions. I, personally, felt rather sad, at this prospective loss of Elliott's inspiring lead, in a field of physiological research in which I had been so fortunate as to find some community of interest with him. But, by an instinct which came naturally to me, and which was only to become stronger through many later years of our association, I was content, in this case also, to accept Elliott's judgement as the right one.

4. *Student and Professor of Medicine*

Arriving at University College Hospital to begin his clinical studies at the age of 29, Elliott would have had to pass through the regular mill, as clerk and dresser in the medical and surgical departments, with students who were his contemporaries in hospital standing, but his juniors, in most cases by many years, and especially in scientific knowledge and experience. He was always ready to mix with younger men, and on a footing of easy friendliness, free from any hint of condescension. On the other hand, he had come from Cambridge with a well-earned reputation, and the staff at U.C.H. was rich in distinguished men who would be eagerly alert for talent among the students, and ready to give a man of Elliott's quality a hand of friendship, and special opportunities for contact with their own ripened knowledge and experience on the clinical side. Victor Horsley, a great research worker and an enthusiastic champion of the cause of experimental research, before he became a great practical surgeon, was retiring from the staff at U.C.H. at the time of Elliott's entry in 1906. John Rose Bradford, however, who had also had a brilliant record of research in physiology, had succeeded Sidney Ringer there as a physician six years earlier; and it is safe to suppose that his influence was more direct and effective than Horsley's in the encouragement of Elliott's own interests in clinical medicine, in its stimulating presentation to students, and in the promotion of its development into an experimental discipline of a special kind, and in its own right. Bradford had in earlier years collaborated with William Bayliss at University College, on the other side of Gower Street, so that he was able to make easier, if that had been necessary, Elliott's friendly reception by, and his opportunities of consultation with, Bayliss and Starling in the Department of Physiology there. When Bradford died in 1935, it was Elliott who wrote the obituary notice of him and his career for the Royal Society (*Obit. Not. Roy. Soc.* **1,** 527, 1935); and it is interesting to observe in it several points at which Elliott seems to recognize hints of analogy between Bradford's career and his own, though in every such case the change of aim and opportunity had come earlier in life for Bradford than for Elliott. Thus Bradford's physiological researches, from 1888 till 1903, when he was 40, had been supported by different outside emoluments, beginning with the George Henry Lewes Studentship for two years; but, already in 1899, when he was only 26, Sidney Ringer had persuaded him to become also his Assistant Physician at U.C.H. 'It will be seen later', wrote Elliott, 'how he strove to extend his clinical knowledge, basing it firmly on medical experience and not on doubtful inductions from physiology and pathology'—in which, none the less, he still continued his laboratory researches. In 1894, at the age of 31, Bradford had been elected to the Royal Society's Fellowship; and in 1907, a year after Elliott's entry at U.C.H., Bradford had become Foreign Secretary R.S., and then, in the following year, Biological Secretary, from 1908 till 1915, when war duties abroad obliged him to retire. Later in this obituary notice, mentioning Bradford's appointment, in 1900, as full Physician in succession to Ringer, at the age of 37, Elliott reflects that 'Somewhere near the age of 40 has often been seen to be a dividing line where men who had shown high promise of powers for research turn aside and give themselves to routine duties. It is usually the man's deliberate, or at times subconscious choice of the way in which he can work most usefully'. And one is tempted to wonder whether, in writing this, Elliott had not his own experience also in the back of his mind; for Elliott had been 41 in 1918, when the war came to an end, and when his own long-term opportunity, as one of the first of the whole-time Professors of Medicine in London's medical schools, had been coming definitely into view.

Another of his seniors at U.C.H. whom Elliott greatly admired, and whose friendship and counsel were to take a high place in his regard, was Wilfred Trotter, brilliant surgeon and distinguished social philosopher, who also became F.R.S. in 1931, and whose obituary notice in the Royal Society's series (*Obit. Not. Roy. Soc.* **3**, 325, 1941) was also written by Elliott. But I am convinced that an even more powerful influence on Elliott's interests in clinical medicine, and especially in the prospect of its development into an experimental discipline, was due to the close friendship which he formed with Thomas Lewis. Lewis was his junior in age by four years; but, since he had entered the hospital for clinical training in 1902, at the more usual age of 21, he was Elliott's senior in hospital standing, also by four years, and had qualified in medicine and held house-appointments under Sir Victor Horsley and Sir Thomas Barlow, before Elliott's arrival in 1906. Such a difference in age had little significance, however, between men, each of whom, though hitherto in somewhat different directions, had shown so remarkable a maturity in their scientific development and interests. They became close friends and, for a time, shared a lodging in Camden Town, with the opportunity each evening, after a frugal dinner, of discussing freely their wide community of scientific interests, problems and ideals, under conditions of which Lewis, was, years later, to give a grimly humorous reminiscence on the occasion of Elliott's retirement. Lewis, however, was already busily engaged in his researches on the heart and circulation, which, after his first meeting with James Mackenzie in 1908, were to extend more and more to direct, experimental observation and recording on the human subject, in health and disease. During those years, from 1906-1910, Elliott had still been fully engaged in clinical studies and qualification, and with medical and surgical house-appointments. And then, with an exceptional promptitude which, in itself, gave remarkable testimony to the impression which his ability had created, he had been elected in 1910 as an Assistant Physician to the hospital, and had therewith come into a more lasting official relationship with John Rose Bradford. And, meanwhile, there had been two events of major importance for future developments in medicine. In 1908 the Royal Commission to deal with medical education in London, in relation to the University of London as a teaching institution, had been appointed under the chairmanship of Lord Haldane; and although the intervention of the war was to delay the presentation of its Report till 1918, it had much earlier become known that its discussions were favourable to the establishment of Professorial Chairs in the principal London medical schools, for whole-time, advanced teaching and research in the clinical subjects. An opportunity, of a kind for which men of Elliott's calibre would soon be required, was accordingly coming into view. And then, in 1909, the foundation of the Beit Memorial Trust, making provision for the Beit Fellowships, offered support on a more adequate basis, and for a much larger number of qualified research workers in the field of medicine, than all the emoluments which had till then been available in this country for this purpose. Thomas Lewis was the first of these Beit Fellows to be appointed in 1909, at a time when Elliott was fully engaged in his house-appointments. Later, when Elliott had become an Assistant Physician to the hospital, and thus had time again to spare for experimental researches, he too became a Beit Fellow, being the 11th in appointment from the beginning in the previous year.

Elliott continued to publish the results of these further researches, mostly from his period as a Beit Fellow, up to the end of 1914, when he went overseas on war duty. All of them are concerned, in some way or another, with the functions of the adrenal glands. Two of them are morphological papers dealing with the cortex and with pathological changes in the glands, while four others, two of these being abstracts in the Physiological Society's *Proceedings*, are concerned with the production of adrenaline in, and its secretion from, the medulla, with the influence upon these functions of impulses in the splanchnic nerves, and with the biological and other technical methods for measuring adrenaline in high dilutions. The observations made in all these publications, however, while characteristically sound and careful in detail, could hardly be regarded as fully maintaining the brilliance of originality and enterprise, which had been so conspicuous in those of his Cambridge period. Anything which Elliott published, then or later, had its own special character of distinction; but this series of publications, between 1911 and 1914, gave somehow the impression of a main concern with matters outstanding from the Cambridge period, and of a clearing of the ground for new interests and different lines of activity. It was not to be expected at that still early date, when practically nothing was yet positively known in detail about the physiological significance of the adrenal cortex, that Elliott would have been able to give full weight to its separate and vitally essential functions, in interpreting the fatal effect of extirpating both the entire glands. In fact, he seems to have regarded the loss of the medulla, and of the adrenaline which it normally secretes, as having an importance in this connexion which later evidence was to exclude, and thus to have been led, in his Sidney Ringer Memorial Lecture (*B.M.J.* **i**, 1393, 1914), to make what seemed to be a reluctant but final renunciation of any further concern, for himself, with the chemical-transmission theory, of which he had been the original proponent ten years earlier. Here is the form of this parting valediction:

'It is always a pleasure, and therefore a temptation, to accept a theory which harmonizes all the facts into a close-fitting plan. But the evidence at present does not justify us in welcoming this simplification.'

Good-bye to it then! And, one feels, good-bye to any expectation which Elliott might so far have retained, of a further renewal of his own direct activity in that kind of experimental research. He was confirming, we may suppose, the plans which he had been laying in a different direction. The whole-time University Chairs in the clinical faculties, to which the Haldane Commission's Report was to give an important place in its recommendations, could already be foreseen with some confidence; and, but for the intervention of the war, they might well have come into being in 1914. When the war came, Elliott promptly joined the Officers' Training Corps, and then, in September 1914, joined his former Chief, Sir John Rose Bradford, as a medical consultant to the British Expeditionary Force in France. During the war he rose to the rank of Colonel and was twice 'mentioned in despatches'; and, at its end, he received the D.S.O. in 1918, and was made C.B.E. in 1919.

Even before the war, Elliott had been in the counsels of some of those who were responsible for the effective beginnings of the Medical Research Council. The original Committee had been meeting since 1913, and making some of the senior appointments to the projected research departments of their principal first enterprise—the National Institute for Medical Research. So far, however, they had made no appointment to the important post of their Secretary, who, as their chief administrative officer, might be expected to have an important influence on their general policies. I had myself, indeed, delayed my acceptance of a research appointment at the National Institute, until I knew who was to hold this key position. And then I learned that Elliott, in this connexion, had brought to the notice of Sir Frederick Gowland Hopkins, who was a member of the Committee, the fact that Walter Morley Fletcher, whom all three of us knew as a distinguished scientist, a great personality and an able man of affairs, was coming to the end of his term as a Tutor at Trinity College, Cambridge; and later, when I heard that Fletcher had been offered and had accepted the Secretaryship, I promptly accepted the research appointment for myself; so that he and I both entered the service of the Committee on 1 July 1914. When the war broke out just over a month later, there was naturally a call for immediate decisions and vigorous actions, to expand and reorientate the Medical Research Committee's then still tentative programme, so as to bring it progressively into effective relation with the new, unforeseen and unexampled needs of the armed forces, and of the whole nation at war. And thus there was very soon clear evidence of the value of the service which Elliott had rendered to medical science and to the nation, when he brought Fletcher's name, and his special qualifications for their Secretaryship, to the notice of the Medical Research Committee. This evidence was further to accumulate steadily during the remainder of Sir Walter Fletcher's all too brief career; and it was natural, and also very fortunate, that Elliott himself was so soon available, from his position in France, to act as a regular and trusted adviser to Fletcher, and through him to the Medical Research Committee, to its permanent research staff and war-time recruits to its service, on all matters concerned with the changing medical needs and problems of the army, during the course of the war. And, from this beginning, Elliott was to retain a relationship of confidence with the Committee, and of special interest in its plans and achievements, from its early development into the Medical Research Council, and then for the whole further period of his own active years, extending even beyond his retirement from his academic and hospital appointments. He was to be appointed foı three full periods in all to scientific membership of the Council—the only instance, so far, of such a total length of service in that capacity; and he continued to be the trusted friend and confidential adviser of those who have followed Sir Walter Fletcher in the Secretaryship, Sir Edward Mellanby and the present Secretary, Sir Harold Himsworth; the latter having been Elliott's pupil, and his successor as Professor and as Director of the Medical Unit, at University College Hospital.

Elliott's record of research interest during the war was concerned with the morbid anatomy of wounds of the thorax, which at that time, of course, had a vital bearing on the consequent problems of function and treatment. With his colleague Captain Herbert Henry, he published a paper on this subject in 1916 (*J. of R.A.M.C.* **27**, p. 525, 1916). And then in 1918, with so many others, he returned to recover the threads of peace-time activity under conditions of rebuilding. For him it brought a new and lasting source of private and domestic happiness, through his marriage to Miss Martha McCosh, of Airdrie, Lanarkshire, whom he had known already as the sister of one of his College friends at Trinity, and had met again with the army in France, where she was on voluntary nursing service; while, on the other hand, his further opportunities in medical science were becoming more definite, through the prospective foundation, at University College Hospital, of the first of London's full-time Chairs of Clinical Medicine. Apart from the Research Unit to be attached to this University Chair, the Medical Research Council had earlier decided to found, at the same hospital, the first of the full-time units for research, in each case on some suitable, chosen range of problems in clinical medicine, which it was planning to establish. The academic Chair, with its own research unit, would obviously involve responsibilities for the new organization and administrative control of teaching and research over a wide range of clinical medicine, and of the scientific disciplines which would increasingly come into its service, with its progressive development into an experimental science in its own right. The Director of the M.R.C. Unit, on the other hand, was to have the opportunity, with a chosen team of specialist assistants, for the intensive development of research on some chosen set of clinical problems, with freedom from any ulterior or formal responsibilities, for teaching or administration. It was obvious that Elliott's claim to be considered for either of these novel and important opportunities would have been a strong one. On the other hand, I do not think that there was any doubt that the most appropriate choices were made, when Elliott was appointed to the Professorial Chair, and his intimate friend and colleague, Thomas Lewis, to the Directorship of the Medical Research Council's Unit.

Elliott's course was thus set and stabilized for the remainder of his active career. Not many years after he became Professor, he began to be hampered by and progressively to suffer from an intractable lumbago and sciatica, which was to make it impossible for him to stand, or even to sit at a meeting, for more than short periods. During the later part of his official life, one became accustomed, at an early stage of a meeting, to see him slip from his chair, unable longer to bear its pressure, and then continue on his knees to take an effective part in the proceedings, with no perceptible diminution of his alertness, or of the cogent wisdom of his interventions. Under such conditions it was not to be expected that he would still be able to bear the physical strain of much direct and personal participation in experimental researches; and his publications during the later years of his activity were, indeed, infrequent. But from the uniform enthusiasm of the reports which were current while he lived, and from the impressive volume of the admiring and affectionate testimony to his personality and his achievement, which was published when the news of his death was received, by distinguished men who had been his pupils and successors, there was abundant evidence of the success and of the lasting effect of his great contribution to the movement which, in these recent years, has been so greatly accelerating the advancement of medical knowledge, by changing its methods from those of slow, empirical accumulation, to those of a rapidly progressive, experimental science. 'Elliott', wrote one, 'was shaping the future of academic medicine in this country, and the many whole-time professorial units in our medical schools today owe much to his guidance and sagacity.' 'The great achievement', wrote another, 'of the later phase of Elliott's life was the practical demonstration that an academic unit, combining teaching, research and the care of patients, could be made to work.' And, again, 'He taught them (his pupils) to deal with the symptoms and signs of disease as the raw material of a scientific discipline, to be observed, measured, and argued about as the starting point for fresh investigations. The generation he trained has succeeded in establishing this point of view, and we are now seeing the fruits of the application of scientific methods to clinical problems'.

Elliott retired from the Chair of Medicine at University College Hospital in 1939, at the age of 62. His wife and he had built themselves an attractive home at Broughton, in Peeblesshire; and years of courageous devotion to the duties of an exacting post, in spite of increasing disability, had more than earned for him the additional rest and comfort which retiring from this major commitment could give him. He returned to the hospital, by request, to allow his colleagues and pupils to honour his 75th birthday, in 1952, by presenting him with his portrait, which he, in turn, presented to the Medical School, in the library of which it now hangs, between those of his intimate friends and colleagues, Wilfred Trotter and Thomas Lewis.

Elliott's regular activities, however, as Professor of Clinical Medicine, had by no means covered the whole of the services which he rendered to University College Hospital, and to other organizations concerned with the support and promotion of medical researches. It was he who, with Sir George Blacker, travelled to the United States of America, and successfully negotiated with the Board of the Rockefeller Foundation for generous endowments to the University College Hospital and Medical School, and, with admirable foresight, arranged that the capital of the gift providing hospital beds should pass to the Medical School, if the Government should become responsible for the beds. His long continued service to the Medical Research Council, and to its enterprises, has already been mentioned. Another service to which he gave many years of voluntary devotion was that of the Beit Memorial Trust, of which, having himself been an early holder of a Research Fellowship, he was to become the Secretary of the Advisory Committee, and eventually, a member of the Board of Trustees. He served, further, on the Interdepartmental (Goodenough) Committee on Medical Schools. And the appointment which he was able to retain longest was that made in 1936 by the Will of Sir Henry Wellcome, by which he and I were appointed as the two representatives of medical science on a Board of five Trustees. The duties of the Trustees were to hold all the shares in the pharmaceutical industry of which, until his death, the testator had been the sole proprietor, and to use the dividends thus coming to them for the support of researches in the medical range of the sciences, as widely defined by the Will, and of museums and libraries concerned with the history of medicine. It was, to me, a matter for great personal rejoicing to find myself once more thus closely associated with my friend and greatly admired colleague of so many years. The early years involved the Trustees in much consideration of the proper interpretation of their functions, as the holders of an independent, charitable Trust. And in the discussions leading to agreed decisions on such matters, with the ultimate approval of the High Court, Elliott's wisdom, his high standards and his keen, generous vision, were of the greatest value to his colleagues.

So too was the staunchness of his support through the difficult period, when the war brought the complete destruction, in one night's bombing, of the central offices of the business, and when the national need further demanded a bold policy of reconstruction, in some directions long overdue. And it was a cause of special satisfaction to his colleagues that Elliott was still able, in spite of his increasing disability, to retain his Trusteeship into a period in which the Trust was free to undertake a fuller discharge of its proper, charitable functions, and that he was still able to travel regularly from Scotland to attend the meetings of the Board, until 1955. Then he finally retired; but he still kept a personal interest in the Trust and its doings, though a series of illness progressively weakened his reserves, until the end came on 4 March 1961.

Apart from the permanent additions to physiological knowledge made by Elliott's early and notably brilliant researches, the influence will long remain of all that his outstanding ability and his devoted enterprise contributed, throughout his career, to the establishment of medicine as a progressive science. And there are many who, as his friends and his pupils, will long cherish the memory of his strong and generous character, and of his attractive and inspiring personality. He was genial as well as impressive in manner, judged an issue quickly and fairly, and spoke with a characteristic clarity and emphasis in giving his opinion. Both in speech and in writing, indeed, he seemed to have a natural and ready instinct for the vivid and effective phrase. Such attributes led many to seek and to trust his advice, readily given, but with no hint of an undue solemnity, or pretention to authority. His talk among friends, indeed, would often sparkle with a gay humour, quick repartee and kindly banter. Elliott will long be remembered by many as a devoted servant to science and medicine, a loyal colleague and a warm-hearted friend.

He is survived by Mrs Elliott and by their three sons and two daughters. None of these have shared or followed their father's special interests in medicine, or in any department of the natural sciences. His sons are engaged in business and the law, and the main interests of both his daughters are in music.

I have been greatly indebted to Mrs Elliott for help in the preparation of this Memoir, and especially for many details of her late husband's ancestry and family connexions, and of his education before he went to Cambridge. Also to the admirable obituary notices in the *British Medical Journal* and the *Lancet*, to which a number of Elliott's friends and pupils contributed, and to that by Sir Harold Himsworth in *Nature*.

For the loan of the photographic portrait, which shows Professor Elliott as he was in 1939, before the second World War, I am also indebted to Sir Harold Himsworth, F.R.S.

H. H. Dale

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R. A. Frazer

ROBERT ALEXANDER FRAZER

1891-1959

ROBERT ALEXANDER FRAZER was born in the City of London on 5 February 1891. His father, Robert Watson Frazer, LL.B., had retired from the Madras Civil Service and had become Principal Librarian and Secretary of the London Institution at Finsbury Circus, whence in the following two decades he produced four books on India and its history, of which perhaps the best known was one published in the 'Story of the Nations' Series by Fisher Unwin, Ltd., in 1895. The family lived at the Institution and Robert was born there.

Young Frazer proceeded in due course to the City of London School where he did remarkably well and won several scholarships and medals. By the time he was eighteen years of age, the City Corporation, desiring to commemorate the distinction just gained by Mr H. H. Asquith, a former pupil of the school, on his appointment as Prime Minister, founded the Asquith Scholarship of £100 per annum tenable for four years at Cambridge. It thus came about that at the school prize-giving in 1909 the Lord Mayor announced that the new Asquith Scholarship had been conferred on Frazer, who was so enabled to proceed to Pembroke College, Cambridge, that autumn.

Frazer, in the course of his subsequent career, had two other formal links with London. In 1911 he was admitted to the Freedom of London in the Mayoralty of Sir Thomas Crosby, having been an Apprentice of T. M. Wood, 'Citizen and Gardener of London'; and in 1930 he was awarded the degree of Doctor of Science by the University of London. The former may or may not have been a pointer to his subsequent ability as a gardener in private life; the latter was certainly a well-deserved recognition of his scientific work at the time.

Pembroke College gave Frazer not only a first class opportunity to develop his mathematical ability—his tutor was the algebraist J. H. Grace, and H. F. Baker, then interested in the matrizant, also played a part in his tuition—but seems also to have brought out two other characteristics for which he later became specially well known among his friends. He was a student with quite unusual powers of concentration and endurance. Throughout the greater part of his life, it was nothing to him to pursue a mathematical problem not only all day, but well into the early hours of the next; and that with a minimum of break for food or conversation. He loved to work on a large unvarnished table rather than a modern business desk; and when visiting the writer in a cottage then lit only by oil lamps he recalled with pleasure the many hours of study at Pembroke alone in his room by lamp light that set a pattern for his future.

The other custom of Frazer's that first showed itself at Pembroke was his agility as a climber. The customary 'vertical' layout of students' rooms in men's colleges around unconnected staircases has always invited the use of unofficial 'horizontal' routes by way of roofs or window-sills, and Frazer soon found that his tall slim build was just right for such journeys. From the walls and roofs of Pembroke to mountain climbing proper was, especially at the time, a natural enough step. The Edwardian era saw a great surge of enthusiasm for mountain climbing among undergraduates; and from this came the first climbers of Everest. Frazer was caught up in the surge and for over a decade devoted his holidays almost wholly to climbing, first in North Wales, Cumberland, and the Isle of Skye and later in Switzerland. So he came to climb with Odell and other famous climbers, who were later to essay Everest; and so, to his great satisfaction, he came to be elected to the Alpine Club of Great Britain in 1921.

Frazer's career at Cambridge won him a First Class in the Mathematical Tripos (part I), and in 1912 he became a Wrangler and Rayleigh Prizeman. But in spite of his mathematical powers, his naturally quiet and studious habits seem to have left little impression on the College authorities, and remembrance of him is much more hazy there than would be expected by those who knew him later in life.

The First World War saw Frazer at the National Physical Laboratory, where he was appointed to the Aeronautics Section of the Engineering Department (now the Aerodynamics Division) in 1914, and this remained his working home until his retirement in 1954. He was at first engaged in theoretical work on viscous flow and in related water tunnel experiments, and though this work did not last long it caught his imagination so that on occasions much later in life he would sometimes, when moving from one field of work to another, express a hankering to return to his first love, remarking that the truth of the matter was that he was 'heart and soul in fluid motion'.

The war saw the growth in this country of a lively interest in rigid airships and Frazer, together with J. R. Pannell, C. W. Duffield, H. Bateman and others of the young men then in the Aeronautics Department, became engaged in a well-known series of full-scale experiments on the performance of airships. This work lasted from 1918 to 1921, during which time Frazer took part in trials of H.M.A. R.29, R.32, R.33, R.36, R.38, and R.80. In spite of the difficulties of such full-scale work under rather primitive conditions, a great deal of knowledge was gained on aerodynamic scale effect and on the drag, lift, and yawing moments that act upon airships. Recalling some of the difficulties, Bateman has remarked: 'goodness knows how many hours we spent going round in circles with the rudders locked at different angles. In R.32 our efforts to correlate model and full-scale by turning circles were shattered when we discovered that the airship—made of wooden girders—had a pronounced bend when any rudder was applied.' In the R.36, which was to have been the first passenger-carrying British airship, on a trial flight circuiting London, Bath and Manchester, lunch for the experimenters was interrupted 'when an elevator collapsed and the airship nose-dived. First, apples rolled from the rear tables towards us, then crockery, then everything that was loose, and finally Captain Scott (who was lunching at a rearward table) came forward grasping the "upright" supports, which were then at about 40 degrees from the vertical, looking like Tarzan of the Apes. Luckily, the officer in the control room had taken prompt action, stopping engines and discharging nose ballast and, after some commotion, we crawled back to base at about 15 m.p.h., our horizontal trim being governed by the movement of men from station to station along the gangway.'

As any who were engaged in airship work will remember, the aspect of early airship operation that seemed to call for the greatest improvement was the matter of launching and landing. Frazer was always keen to try to overcome the difficulties here, and designed, with L. F. G. Simmons, a three-wire mooring system which was patented but which, beyond a few trials at Pulham, Norfolk, was never used in practice. The above-mentioned Captain Scott, who afterwards became, as Major Scott, the best known of British airship commanders, was later to solve most of the launching and landing problems by the introduction of the rigid mooring mast, of which the British example at Cardington was for long a landmark in Bedfordshire.

This phase of Frazer's work was brought to a sudden end by the catastrophe to the airship R.38 over the Humber in 1921. Frazer himself was not on board at the time, but his colleagues Pannell, Duffield and Bateman were, and only the last named survived. It was appropriate that in due course the R.38 Memorial Prize was founded by the Royal Aeronautical Society and that Frazer was in 1925 the third recipient for his essay on full-scale work on rigid airships.

In 1921 a number of young Oxford graduates were planning an exploration to Spitzbergen, and Frazer, who knew some of them through his climbing and ski-ing activities, was invited to join them. As a result there began in 1921 the first of his three visits to Spitzbergen, culminating in the relatively substantial exploration of 1924 that is well described by its leader, George Binney, in his book *With seaplane and sledge in the Arctic* (Hutchinson, 1925). This Oxford University Arctic Expedition comprised 25 men, of whom three (including Frazer) had already been on the smaller 1921 and 1923 expeditions. Three sledging parties set out on different routes and duties from the base at Liefde Bay, at the entrance to Wahlenberg Bay, North East Land; and one of these was led by Frazer. He served also as a surveyor to the expedition.

Frazer was in charge of the central sledging party and, as Binney has said, 'He was the mountaineer of the party and with his previous year's experience of sledging in New Friesland it was natural that he should be in charge. It was equally natural that the other three (in the party) should rely implicitly on his good judgement, whether in choosing the best route or in leading across badly crevassed country. To use a metaphor of the Stock Exchange, Frazer was a gilt-edged security, and, while stock of this nature usually gives a small return, Frazer's survey work has been one of the main dividends of all three Oxford Expeditions.'

This is not the place to discuss the direct achievements of the expedition, but it is of scientific interest to look down the list of names of these young explorers. Among them are no less than four who have since become Fellows of the Society and one of these (Dr H. Florey, medical officer to the 1924 expedition) is now our President. Frazer was not the only man released for this work by the National Physical Laboratory; E. F. Relf served officially as Physicist, Surveyor, and Assistant Wireless Operator and unofficially and characteristically as an invaluable jack-of-all-trades as well.

In 1922 Frazer published a paper on the topographical work of the 1921 expedition and another of his colleagues on that occasion (N. E. Odell) gave a paper on the geology of the expedition. As Professor J. S. Huxley (himself a member of the 1921 expedition) said in the discussion of these papers by the Royal Geographical Society, the expedition gave 'an exceptionally good opportunity of studying, within the boundaries of one small country, what the state of the British Isles must have been at different periods during their emergence from glacial conditions'.

In 1923, just before his second Spitzbergen journey, Frazer married Alice Goldie, herself then on the staff of the National Physical Laboratory, and although in later years his more exciting climbing and exploring activities drew to a close, he and his wife still enjoyed a long series of holidays among the mountains and in the snows of the Alps and elsewhere.

During his early years at the N.P.L. the current interest of aeronautical engineers centred on the study of laminar and viscous flow. L. Bairstow and R. V. Southwell, successive Heads of the Aerodynamics Division, were both drawn into these flow problems and with them, a number of the younger members of their staff. Frazer became specially interested in the theory of viscous flow around idealized bodies such as circular cylinders, an interest that culminated in his first paper to the Royal Society. It was during this time, according to his close colleague, J. L. Nayler (who was later the Secretary of the Aeronautical Research Council for many years), that Frazer first saw the possibilities of matrix methods and set about making himself a master of the then little known technique. From his work in this field, also, came his deep understanding of vortex motion that later bore fruit in his work on the aerodynamic derivatives of flutter theory.

The occurrence in 1916 of violent oscillations of the tail of a new Handley Page bomber aircraft led to the earliest study of what is now known as 'flutter'. As a result, this trouble was attributed to antisymmetrical oscillations of the two halves (unconnected) of the elevator coupled with torsion of the fuselage. From this investigation, originally reported on by Lanchester, Bairstow and Fage, sprang the long series of researches on aeroelasticity at the N.P.L. led by Frazer. His first publication in this field, 'An investigation on wing flutter', appeared in 1926. This discussed the problem of the flutter of a biplane in terms of wing flexure and aileron motion, and developed and widened an earlier exploration by Bairstow in 1925. Using linear equations of motion in the form originated by Bryan and developed by Bairstow and Nayler for the discussion of the rigid body stability of an aeroplane in flight, Frazer was able to make an approximate estimate for critical flutter speed that agreed reasonably with practical experience on the aircraft concerned. He showed too that for such a biplane mass-balancing of the ailerons would remove the tendency to flutter.

W. J. Duncan joined him at this time and a variety of wing flutter problems were investigated by one or the other in rapid succession, and as a result came the A.R.C. *R. and M.* 1155 ('The flutter of aeroplane wings') a joint work that has ever since been used and known among British workers in aeroelasticity as the 'flutter bible'. Frazer was now at the top of his form; there followed, either by himself alone or with others, no less than eighteen papers in the field of flutter between the years 1928 and 1946; and derived therefrom some six papers on mathematical matters of wider scientific interest.

Reviewing the outcome of this flutter work—undoubtedly the period of Frazer's greatest achievements—there appear to the writer a number of advances both to the subject and to applied mathematics that can fairly be attributed largely to Frazer himself, though much of his work, to the outside observer at least, was so closely linked with that of Duncan and A. R. Collar as to make separate allocation difficult.

In the first place, in his earliest flutter work, Frazer appears to have been so impressed with the disabilities of Routh's stability criteria for small motions as to be one of the first to seek stability tests of a more convenient and comprehensive kind. Work on this with Duncan during the first two years of their collaboration resulted in their Royal Society paper on 'Test functions' for the stability of small motions. These functions were much used during the next decade, and still remain in use among some British flutter workers.

A second development largely originated by Frazer, though from the literature attributable only to Frazer and Duncan jointly, is the notion of representing an elastic wing, with its infinite number of degrees of freedom, by a 'semi-rigid' one with a finite number (small) of degrees of freedom selected to suit the flutter problem concerned. It might be argued that such an idea was inherent in the Rayleigh–Ritz method for finding the natural frequencies of an elastic body, but Frazer and Duncan developed and formalized their semi-rigid approach in physical and mathematical terms to treat of problems beyond the aims and scope of the original Rayleigh–Ritz process. It could now be argued, perhaps, that in its original form its use has largely been relegated to the teaching and understanding of flutter problems rather than to their solution in practice; but even if this were generally true the place of the semi-rigid idea in the British history of the subject is a major one.

The third aspect of his flutter work has been of more widespread and general value, and is still very much alive, not only in aeronautical science, but in engineering science generally. The application of matrices to engineering problems in this country was largely initiated by Frazer, though Duncan and Collar of course made substantial contributions as the N.P.L. work progressed. The whole led to the writing and publication of *Elementary matrices* by Frazer, Duncan and Collar, in 1938. This almost immediately became a standard textbook among applied mathematicians and engineers, not only in this country but internationally, and has, somewhat surprisingly having regard to its *ad hoc* and non-didactic form, remained so ever since. Frazer's work has thus had a major effect upon subsequent work on vibration problems, both in their aerodynamic and structural aspects, particularly in this country.

A further point regarding this main field of Frazer's work relates to his treatment of the aerodynamics of aeroelastic problems. Starting with the notion of constant aerodynamic coefficients as in the linear theory of aeroplane stability, he and his colleagues successfully built into the machinery of flutter speed prediction the use of aerodynamic derivatives that not only varied with forward speed but also provided for the effects of vorticity as first analyzed in this country by Glauert. And although it was left to Duncan and Collar, H. A. Jahn and W. P. Jones to expand this process as a working tool and to allow first for compressibility and later for supersonic flow, the framework owed much to Frazer himself.

A last point may be made here regarding Frazer's work on linear systems. He became interested in the effects of solid friction and produced, with Collar, a series of unpublished A.R.C. papers that were later incorporated in the last three chapters of their book with Duncan. This describes the first comprehensive treatment of this dynamical problem of 'ankylotic' motion—Frazer's apt term for motion in which friction causes the 'sticking' of a degree of freedom.

By the end of the Second World War this major and most fruitful period of Frazer's life was at an end and was recognized by his election to the Fellowship of the Royal Society in 1946. Thereafter, like a number of his colleagues in the flutter field, he turned his attention to other, though related, problems. The end of the war saw the revival of a scheme for building a suspension bridge across the Severn at the narrows above Bristol to serve as part of a major motorway to South Wales. But by this time suspension bridge designers were acutely aware of the danger of aerodynamic oscillations of such bridges in cross-winds, for their world, and indeed the public at large, had been severely shocked in 1940 by the spectacular and much publicized collapse of the Tacoma Narrows Bridge in America. Scientific investigations as to the causes of the catastrophic oscillations that then occurred were already under way in America and interest was arising in this country also. It thus came about that, to help in the design of the proposed Severn bridge, Frazer was called upon by the Ministry of Transport to undertake extensive model experiments related to the proposed design.

Frazer entered upon this work with enthusiasm and soon had an excellent team, headed by Scruton, at work. All the characteristics that his earlier colleagues had come to expect of him were brought to bear. After a most thorough (and original) examination of the dimensional similarity conditions to be met by such a model, he launched upon the design of a large model (some 50 feet long) of the proposed bridge, and also guided the design of a special low-speed wind tunnel to test this specimen in an aircraft hangar at Thurleigh, near Bedford. At the same time he himself developed a very general theory—in matrix form—for predicting the natural frequencies of a suspension bridge with a deck constructed to have substantial torsional stiffness (then a new feature of suspension bridges).

Whilst this work was in hand, a whole series of sectional models of different designs of bridge deck were constructed and tested in the N.P.L. tunnels to examine the effects of different design features upon their aerodynamic stability when given freedom, elastically restrained, in vertical and torsional motion. From this he was able to confirm, and give some quantitative judgement upon, the tendency for 'solid web' plate girders to give rise to oscillations associated with eddy-shedding under cross-winds, and to establish the relative freedom from such oscillations of decks with open-web trusses for their main girders. From this work too came a demonstration of the value of longitudinal gaps or slots in the floor of a bridge deck to minimize any tendency to oscillate under cross-winds.

With this knowledge in hand, the first designs for the bridge were modified to incorporate the best anti-oscillation features so found, and the large model was completed accordingly. In the actual testing of this model at Thurleigh, Frazer's ability to lead a team, first demonstrated in his sledging in Spitzbergen, came again to the fore. Housed under primitive conditions in R.A.F. huts on a deserted and lonely airfield, Frazer's team set about the job of calibrating its special tunnel and testing its huge model with a thoroughness and happy spirit that struck every visitor. At any moment Frazer was just as capable of cooking breakfast for the party with a minimum of equipment as of discussing the niceties of ensuring the proper aerodynamic form of details on the model or of satisfactory flow in the tunnel. The whole job was a *tour de force* that was a triumph for the limited staff, money and time available, and that provided a sound background for the design of the Severn, and now the Forth, Suspension Bridge.

With this work on suspension bridges Frazer's scientific career effectively closed; and so it came about that although he gave occasional thought during the decade following the war to his first love—fluid motion—he was never able to make any substantial contribution in that field. He was not alone in this; others in the early group under Bairstow were, like Frazer, drawn away by circumstances into other activities and never able to return. So the major advances in the field of fluid mechanics that they had hoped to lead fell largely to the hands of other people.

On his retirement in 1954, Frazer energetically devoted himself to gardening on quite a large scale. At his home at Ockham he created, almost from scratch, a garden designed and planted in a manner worthy of Kew; and all his friends viewing his hard work there and his tall thin frame apparently still so vigorous, thought and hoped he would have a long and happy retirement in the company of his wife and children. But it was not to be; he fell ill during the autumn of 1959 and died on 10 December that year at the age of 68.

Outside the committees of the Aeronautical Research Council, Frazer never took a very active part in scientific bodies. He was a Fellow of the Royal Aeronautical Society and of the Institute of Aeronautical Sciences; but his energies as a committee man were mainly devoted to the Mechanics Committee, and its Oscillation Sub-Committee, of the A.R.C. He was a member of both of these when they started and was particularly valued and respected by all the members of the Oscillation Sub-Committee, to whose affairs he contributed so wisely over the years.

As will have been gathered from this account of Frazer's life and work, he was a man of many admirable qualities. If what has been written makes him appear to have been an excellent leader of a small team of scientific workers, this is true; but he was more than that, he was a lovable colleague of older as well as younger men; and all who worked alongside him enjoyed his quiet humour, his unruffled good temper, and even his habit of perpetually relighting his pipe with matches. In his work he was always a strict mathematician first, seeking general and accurate rather than qualitative or approximate solutions, and only turned to experimental and engineering approaches second and with less assurance; in his person he was always the best of friends.

Frazer leaves with his widow, one son and two daughters. To the former I am greatly indebted for information concerning her husband's early life. I have also to thank especially Professor A. R. Collar and Mr A. Fage, F.R.S., for much helpful information and comment during the preparation of this memoir. I must thank too Frazer's old colleagues, E. F. Relf, F.R.S., J. L. Nayler and H. Bateman, and his younger colleague, W. P. Jones, for their valuable letters and papers on particular aspects of Frazer's career.

Finally, it is to be recorded with regret that this memoir has been written by myself only because Frazer's closest scientific friend, Professor W. J. Duncan, F.R.S., died before he could do more than collect information upon which to base a memoir to be written by himself. I have been much indebted to the material that thus came to hand.

A. G. Pugsley

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W. E. Garner.

WILLIAM EDWARD GARNER

1889-1960

PROFESSOR W. E. GARNER died on 4 March 1960 at his home at Westbury-on-Trym, Bristol, where he had lived for over thirty years.

Garner was born at Hugglescote, Leicestershire, on 12 May 1889, the eldest son in a family of three sons and a daughter. His younger brothers also attained distinction in the scientific world: Sir Harry Garner, after notable achievements in the field of aircraft research and development, became Chief Scientist to the Ministry of Supply, and F. H. Garner was for many years until his retirement in 1960 Professor of Chemical Engineering in the University of Birmingham.

After attending Market Bosworth Grammar School (1902-1907) Garner went to Birmingham University (1908-1913). He was awarded an 1851 Exhibition in 1913 and proceeded to the University of Göttingen to work with Professor Gustav Tammann. Garner's research at Göttingen was curtailed by the outbreak of the First World War and he only managed to get out of Germany in time. In the University of Birmingham he carried out research under Professor P. F. Frankland and it is interesting to recall that he began his scientific life as an organic chemist—his first publications being on the preparation of oximes, amines and the reactions of thionyl chloride with lactic acid and ethyl lactate. It was undoubtedly his year at Göttingen which influenced much of his subsequent research. Tammann—who held the Chair of Inorganic Chemistry—was then surging ahead with his fundamental investigations on the internal pressure of solutions, equilibria between crystalline and liquid states, particularly melting and nucleation, and crystallization of inorganic and organic liquids and metals, and he had gathered around him some twenty to thirty research workers from all parts of the world. In this atmosphere Garner met many distinguished European physical chemists and was in close contact with the development of the new and growing branch of chemistry—known as physical chemistry.

On his return to England in 1914 he joined the scientific staff of Woolwich Arsenal where he carried out some outstanding research work with Sir Robert Robertson on the calorimetry of high explosives. Garner's experience at Woolwich (1915-1918) was later to be invaluable to him when he became an adviser to the Ordnance Factories, and later Superintendent of Chemical and Explosive Research in the Ministry of Supply during the Second World War.

On leaving Woolwich Arsenal he was appointed Assistant Lecturer in the University of Birmingham (January 1919) and later Assistant Lecturer at

University College, London (October 1919). From that time dated his close and happy association with Professor F. G. Donnan and a fruitful period of research which was to have a significant influence on developments in physical chemistry. Garner had a wide interest in, and deep knowledge of, many branches of physical chemistry and was only happy when following several developing lines. It was during his University College period that he began his studies on heterogeneous catalysis and heats of adsorption, the kinetics of reaction in gases and in solution and in particular the study of explosive reaction in gases, and the ionization and radiation from flames.

In 1927 Garner was appointed to the Leverhulme Chair of Physical and Inorganic Chemistry in the University of Bristol, and until his retirement at the age limit in 1955—except for the war period—he carried out a series of experimental studies of far-reaching practical and theoretical importance. He began a wide and systematic study of the kinetics of solid reactions.

Garner's work was interrupted by the Second World War and immediately on its outbreak he established an extra-mural research team in the University of Bristol to assist the Government Ordnance Factories in explosives and munitions research. This large team continued for the duration of the war and was of invaluable help to the production factories and in developing new explosives and other essential war materials. Although never losing contact with the work of this group, Garner moved to Fort Halstead in Kent in 1943 to become Superintendent of Chemical and Explosives Research for the Ministry of Supply. He later became Deputy Chief of Armament Research and then later Chief Superintendent Armament Research. His enthusiasm, wise guidance and inspiration were of paramount importance to the war effort. He served on very many high-level committees and was associated with many notable developments in new armaments and munitions. His services were recognized in 1946 by the award of the C.B.E. During the whole of the period that he was at Fort Halstead Garner would return to Bristol on the Friday evening, give his lectures on Saturday morning and attend to departmental matters before returning to London on the Sunday afternoon.

On the cessation of hostilities Garner returned to his University work in Bristol, although until his retirement he was actively engaged in the work of the Scientific Advisory Council of the Ministry of Supply and other Government committees. During the post-war period he built around him in Bristol one of the strongest research groups in the country, and continued with increasing vigour his studies of heterogeneous catalysis.

Garner was a man of charm and kindness who won the affection and admiration of all who came into contact with him. His unselfishness and sincerity made him the most likeable of men and he was always ready with encouragement, advice and assistance to his students and colleagues. He was a quiet, unobtrusive man, entirely devoid of personal ambition; devoted to his work whether in the laboratory or the councils of the University. But these qualities did not obscure the greatness of the man. He was an enthusiastic and inspiring leader of research, conscientious in the discharge of his duties and an adherent of the best traditions of science. His interests were wide; he was a collector of paintings and china and had a critical appreciation of art. He was knowledgeable on many subjects—he had a good library of books on the history of chemistry; he was very interested in period furniture and he always enjoyed an hour or so in the sale-room. But above all, he was an enthusiastic gardener and it was a great thrill to him to raise a rare seedling or to produce some unusual fruit. These hobbies were a great comfort to him in his retirement and in recent years he extended his greenhouses and took up the growing of orchids. The care of his plants was a major concern to him during his illness.

He was a man of great tenacity and courage. This characterized his scientific work and everything else he tackled; no problem ever daunted him. When I visited him when he was in hospital he was busy studying catalogues of wheel-chairs since it was thought that he would be able to walk only with difficulty. But despite the doctors' fears Garner, with his indomitable courage, was soon on his feet again and for a short time before his final illness he visited the Staff House of the University for lunch—a pleasure he enjoyed during the five years of his retirement.

He had a wide circle of friends and those who had the pleasure of knowing him valued a friendship which will endure as a cherished memory of a lovable character.

Garner was a well-known figure at scientific gatherings and although not outstanding as a lecturer he was at his best in presenting his own experimental discoveries or introducing some new idea which he was then developing. Scientific societies and Government committees made great calls upon his time. He was President of the Faraday Society (1945-1947) and served as a member of the Council of the Royal Society and of the Faraday Society. He was a member of the Scientific Advisory Council of the Ministry of Supply for almost the whole of the period 1942-1960 and had been associated with that body since its inception. He was also Senior Scientific Adviser for Civil Defence in the South West Region.

He received many honours: he was elected to the Fellowship of the Royal Society in 1937; was a Fellow of University College, London, and was made a Commander of the British Empire in 1947. He was well known abroad and was an honorary member of the Polish Chemical Society, a correspondent councillor of the Patronato 'Alfonso el Sabio', Madrid, 1959. He was a member of the Joint Services Mission to the U.S.A. and Canada 1948.

Combustion researches

When Garner joined the staff of Woolwich Arsenal during the First World War the theoretical and experimental facts underlying combustion and explosion in solids were not well established. The available experimental data were incomplete, especially in respect of the volume of gases and water formed, and reliable values of the heats of detonation and effect of confinement on detonation were not available. In collaboration with Sir Robert Robertson, Garner began a systematic investigation of the determination of the heat and volume of gases produced when high explosives were detonated under standard conditions of confinement and compression in order to obtain a measure of the energy developed, the nature of the gases evolved and the chemical reactions which occurred in the period after explosion. The development of the necessary calorimetric procedures and the results for a number of military explosives stand among the most valuable and important contributions to this aspect of military science.

It was his war-time experience, together with his earlier interest in catalysis, that induced Garner on returning to the University to embark on a systematic study of explosion in gases, particularly the effects of small additions of other gases to the radiation emitted. He made the significant observation that the radiation of the 'dry' carbon monoxide flame decreased by the addition of small amounts of hydrogen or water and that an abrupt fall occurred at a critical percentage of hydrogen. Parallel with these studies he made detailed investigations of the spectra of explosion in gases, infra-red emission from flames and ionization phenomena in gaseous explosions. This series of investigations taken together form a significant contribution to explosion phenomena and the elucidation of the reaction occurring in flames.

Thermochemical measurements

From the time of his association with F. G. Donnan, Garner was keenly interested in colloidal and biological problems and especially the possibility of interpreting these phenomena in physico-chemical terms. As early as 1925 he was writing on the mechanism of muscle contraction and it was this interest which led him into the fields of the study of adsorptions and the properties of substances related to the fats. His publications on the melting points and heat of crystallization of fatty acids and their esters in which he discovered the alternating relationships constitute one of the most fundamental and systematic investigations in this field. The relationship between the melting point T_F of the lower chain paraffins and molecular weight established with van Bibber and King has recently been extremely valuable to polymer chemists working with the newer polyolefins.

Solid state and heterogeneous catalysis

Since the days of Gibbs, Roozeboom and Tammann the study of heterogeneous systems has had a long development. The modern view is not essentially different from that of Tammann, who was a pioneer in many fields, but is governed by ideas which possess greater precision than was possible fifty years ago. In these studies the newer knowledge gained regarding dislocations, lattice defects, and energy levels in the solid state find direct application. It was not unnatural therefore that Garner, who did his post-doctoral research under Tammann, should become one of his disciples and adopt aspects of the kinetics of heterogeneous reaction as his principal field of research. He recognized the three-dimensional aspect of reactions in multi-phase systems. He expanded the two-dimensional picture developed by Langmuir and others of catalytic reactions occurring in absorbed films to one of penetration of the solid in some depth. The kinetics of chemical reactions accompanied by a change of phase are of much greater complexity than homogeneous reactions due to diffusion processes, interface reaction and the creation of new phases. Garner made a wide and systematic study of the latter two processes. His investigations fell naturally into two groups. (a) Endothermic solid reactions such as the dissociation of hydrates and carbonates in which dissociation and recombination processes occur simultaneously. These reactions provided information about the mechanism of interface reactions and nucleation processes. (b) Exothermic reactions of the kind $A_{\text{solid}} \rightarrow B_{\text{solid}} + C_{\text{gas}}$ and in particular the azides and fulminates. Garner showed that the surface reaction which occurred produced lattice vacancies which may be localized near surface discontinuities, may spread over the whole surface or diffuse into the interior. He showed that the nuclei of the new solid phase was probably created within aggregations of lattice vacancies. In carrying out these investigations he developed a number of fruitful experimental methods, including pressure-time measurements, loss of weight using a micro-balance, and microscopic examination of surfaces of decomposing crystals—especially useful in studying nucleation.

Parallel with these researches he continued his interests in catalysis and chemisorption on oxide surfaces. As early as 1924 he recognized the importance of measurements of heats of adsorption and developed and continued to perfect calorimeters for their measurement. The distinction between chemical and physical adsorption was clearly brought out in the paper by Garner & Kingman in 1931, the same year that Taylor & Williamson, and Benton & White proposed the existence of the two types of adsorption. In his extensive studies of the chemisorption of oxygen and carbon monoxide on metal and oxide surfaces Garner realized the full significance of the electronic structure of the solid in catalysis and kinetic processes at surfaces. Although essentially an experimentalist he had a profound knowledge of modern progress in solid state physics and was a pioneer in applying these new developments to heterogeneous reactions. His measurements of the heats of adsorption on zinc, manganese, chromium and copper oxides and the elucidation of the reversible and irreversible chemisorption of gases on these oxides and the interpretation of the experimental results in terms of lattice defect theories provides a model example of the application of many disciplines to the solution of the important problem of the understanding of catalytic reactions at surfaces.

After his retirement in 1955 Garner organized a symposium on chemisorption at Keele, Staffordshire, which was published by the Chemical Society, and edited a large volume on *The chemistry of the solid state.* Most of the contributors to the latter volume were former students or colleagues at Bristol —others were close scientific friends—and this work, which presents the broad basis of progress in solid state chemistry, is very much a tribute to Garner's inspiration and guidance.

C. E. H. Bawn

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P. A. Gorer

PETER ALFRED GORER

1907-1961

PETER ALFRED GORER, pathologist, immunologist and geneticist, died at the age of 54 at the height of his scientific powers. His position in science is not difficult to define: he was the world's foremost authority on the serology and serological genetics of tissue transplantation. His career illustrates 'the often commanding position in research that is gained by an investigator who has mastered two cognate fields of knowledge and sets himself to find and tie the threads that can unite them'.*

Peter Gorer was born in London on 14 April 1907; except for one most rewarding sabbatical year in America, he worked in London all his life, first at University College, then at the Lister Institute, and then finally at Guy's Hospital. His father, Edgar, a keen sportsman and well-known authority on Chinese ceramics, was drowned in the sinking of the *Lusitania* in 1915; his mother, born Rachel Alice Cohen, died in 1954, aged 82. On his father's side, though remotely, Gorer had both Dutch and Russian ancestors; his mother's family were partly Jewish, though one great-grandparent was the daughter of a Devonshire farmer. The Cohens had mainly to do with shipping, but one uncle, Jack, was an able designer and the founder of the Colchester Lathe Company, and another, Alfred, after whom Peter was named, was rated a promising mathematician when he died of leukaemia at an early age. Gorer had a younger and an elder brother who both survive him—the younger, Richard Benjamin; the elder, Geoffrey Edgar, the anthropologist, who became head of his family at the age of 10 and who acted the part on more than one occasion to Peter's great advantage.

Peter was a slow developer. At the age of five, the eminent physician later to become Lord Horder recommended his entering a school for backward children, and when the two met each other much later on, during medical examinations, Gorer had the pleasure of reminding Lord Horder of the circumstances under which they had met before. Horder's advice was not taken: Gorer went to day school at St John's House in Hampstead and then became a boarder at the Golden Parsonage, Gaddesden Row, Hertfordshire. Though he described himself as an undistinguished scholar and by no means so far advanced as his brothers had been at school, he passed into Charterhouse without difficulty. Indeed, much of his allegedly slow development as a child was the effect of a passionate and perhaps also dreamy absorption in natural history, which used up some of the energy that might have gone into school work. It was at Charterhouse that his thoughts first turned towards becoming a scientist—and not surprisingly, for in his day the Science Under-Sixth was in charge of two outstanding teachers, J. C. Holmes (physics) and J. C. Thomsen (chemistry), who did their utmost to encourage their pupils to use their wits. Every Monday, Thomsen set a paper which had to be answered during the week and handed in for correction on Friday; any books could be consulted and any help enlisted, provided its source was acknowledged when the work was handed in. The Chemistry Laboratories were open outside working hours, and a boy with a mind to it could try his hand at a few simple organic syntheses with an experienced technician looking on to make sure he came to no harm.

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* Professor George Payling Wright in his tribute to Gorer in *The Lancet* of 20 May 1961.

Ideas of an orthodox professional career must have been impressed upon him at some later stage of his school life, for Gorer entered Guy's Hospital as a dental student in September 1924. The impression could not have been lasting, for he transferred to medicine within three months. Biology had not been taught at Charterhouse, so Gorer was obliged to take his First M.B. examination before proceeding to anatomy and physiology; he passed easily enough, and was greatly encouraged by the Professor of Biology, T. J. Evans, a learned and versatile man with a reputation for recognizing talent in his pupils. He passed his Second M.B. with equal ease, though he failed to get the Distinction everyone expected of him and was not top of the Guy's entry. Nevertheless he interrupted his orthodox medical career by taking the Special Honours course in physiology for the degree of B.Sc. During this period he twice attended courses of comparative physiology given by C. F. A. Pantin at Plymouth, and even tried his hand at research into the neuromuscular mechanisms of lobsters. Of much greater significance for his later career was the fact that at Plymouth he read for the first time T. H. Morgan's *The physical basis of heredity:* the impression it made on him was life-long. In London he worked under the guidance of the Professor of Physiology, M. S. Pembrey, on respiratory changes during hibernation, and together they published a short note, his first paper (1), in the *Proceedings of the Physiological Society* of 1929. Pembrey thought very highly indeed of Gorer; it must have been he who suggested to Munro Fox that Gorer should be invited to write a *Biological Review*—a distinction that does not often come the way of an undergraduate—on 'The physiology of hibernation' (2). It is a good review, with a sense of historical depth unusual in a beginner's writing: the citations of literature go back 100 years, and Gorer had obviously read almost all the papers he refers to. No wonder Pembrey was greatly disappointed when, on graduating B.Sc. in 1929, Gorer got only a second class degree. Gorer had not the temperament of a 'good examinee' and the asthma that plagued him from early childhood invariably got worse towards the eve of examinations.

Gorer now resumed his medical training. In his clinical years he became deeply interested in pathology and cancer research, and worked for his own interest and amusement in the bacteriology department; he became great friends with Professor G. W. Nicholson, an Irish-German pathologist widely read in philosophy and with an unusually broad grasp of the biological foundations of pathology. During his final examinations he had an altercation—on genetics, of all things!—with the examiner in obstetrics and gynaecology. He failed. Gorer met the same examiner again at his second try, but this time the oral examination passed off quietly and he qualified M.R.C.S., L.R.C.P. in 1932.

By now, not unexpectedly, Gorer was fully resolved to enter a career in scientific medicine; to qualify himself, he had the unusual good sense to study genetics under J. B. S. Haldane at University College, London. There he became friends with D. M. S. Watson and E. J. Salisbury, the Professors of Zoology and Botany, and with G. R. Cameron and G. Payling Wright, future leaders of British pathology, the latter to become head of the department in which Gorer worked when in due course he returned to Guy's.

University College was not at that time materially equipped to allow Gorer to develop his research in the pattern that was beginning to take shape in his mind. In 1934 he joined the staff of the Lister Institute, where he remained until 1940. There he was equally lucky in his colleagues; the staff was not large, but among its members were five others who were elected in due course into the Fellowship of the Society: Alexander Todd, Muriel Robertson, W. T. J. Morgan, D. W. Henderson and F. Bergel.

In retrospect one can see that a man of Gorer's ability and special qualifications might have followed one or other of two divergent pathways of research. He might have applied his knowledge of genetics to the classical problems of immunology—that is, to the problem of resistance to foreign substances of organic origin; or he might have become an immunological geneticist, a man who used immunological methods to study the pattern and the inheritance of differences between individuals. In practice, Gorer began by following both pathways of research, and ended in a direction that in some sense bisected the angle between the two. At University College (surely under Haldane's influence) his first enterprise was to try to find out if the red cells of different mice fell into distinct antigenic groups, analogous to the blood groups of human beings. When he began his work, no such differences were known. When death brought it to an end, he had amassed a wealth of evidence about antigenic variation in mice that underpins the entire theory of tissue transplantation.

Gorer's first discovery (3) was that a normal serum from a human being of group *A* (himself, in fact) distinguished two, perhaps three kinds of red cells in mice: the red cells of *A*-strain mice were strongly agglutinated by his serum; of *CBA* mice, feebly; and of *C57* mice not at all.* The results of some simple crosses suggested that the property of being agglutinable by his serum behaved as a simple Mendelian dominant. Gorer now (4) substituted a rabbit antiserum for his own serum—an antiserum formed by the deliberate injection of the red cells of mice into rabbits. Three antigenic types could now be

* I allot these symbols in retrospect. Gorer was at first uncertain of the history of his inbred strains, and described them wrongly in some of his early papers.

recognized with certainty, and later (5) he added a fourth, though certain references to variations of 'avidity' portend the complexities later to be revealed in Histocompatibility locus H-2. Among his three strains the three principal antigens were distributed thus: antigen I was shared by *A* and *CBA*, and antigen III by all three; antigen II was represented strongly in *A* and weakly in *CBA*.

To have discovered antigenic variation among the red cells of mice was a most creditable accomplishment for a beginner in a difficult field of research. There now followed, in successive years, two papers of the utmost importance (6,7), the first written when he was only 30. Their subject is the relationship between the antigenic composition of tumours and the properties that rendered them 'transplantable'—that decided whether, upon inoculation into mice of other strains, the transplanted tumours would grow until they killed the mice that bore them or, on the contrary, would grow for a matter of days and then dwindle and disappear.

Gorer studied the transplantability of a sarcoma that had arisen 'spontaneously' in an *A*-strain mouse. The tumour did not grow in *C57* mice, but grew in all mice that had an *A*-strain parent, viz: in *A*-strain itself, in the F_1 progeny of the cross ($A \times C57$) and the R_2 progeny of the backcross ($F_1 \times A$). Among the mice of the F_2 generation and of the backcross ($F_1 \times C57$) some accepted the tumour and others did not; the proportions in which the two classes occurred suggested that the differences between them depended upon segregation at two genetic loci. So much was understandable enough, for C. C. Little and E. E. Tyzzer had already defined the general genetic rules that governed the transplantability of tumours, and the more recent work of J. J. Bittner and G. D. Snell had given their conclusions added weight.

Gorer's great innovation of principle was to correlate susceptibility or resistance to the growth of the tumour with the presence or absence of the antigen known to distinguish *A*-strain mice from *C57*s, viz: Antigen II. He found that the possession of Antigen II was not a property peculiar to red cells; ordinary tissue cells, normal or malignant, also possessed it, and more abundantly; and the transplantation of his sarcoma into *C57* mice did in fact excite the formation of an antibody directed against Antigen II—the first 'iso-antibody' to be discovered in mice. All mice that lacked Antigen II rejected the tumour; of those that possessed it, about one quarter in the F_2 generation and one half in the *C57* backcross generation also rejected the tumour. Two main factors must have been at work, then, in deciding whether or not a mouse would accept a tumour of *A*-strain origin; and one of them was almost certainly Antigen II.

Gorer was now in a position to reformulate the genetical theory of tumour transplantation along immunological lines. His predecessors had been inclined to speak of genetic factors the possession of which conferred *susceptibility* to the growth of foreign tumours; Gorer saw that, though susceptibility and resistance were opposite sides of same coin, it would be more in keeping with immunological theory if the possession by the tumour of Antigen II were said to excite *resistance* in mice that lacked it. He put the matter thus (7):

> 'Normal and neoplastic tissues contain iso-antigenic factors which are genetically determined. Iso-antigenic factors present in the grafted tissue and absent in the host are capable of eliciting a response which results in the destruction of the graft. Under special circumstances the response may not be elicited or the grafted tissue may not be destroyed thereby. Antigenic differences between normal and neoplastic tissues are not normally capable of stimulating a defensive reaction.'

The immunological interpretation of susceptibility and resistance to the growth of transplanted tumours was not, of course, new, but it was moribund for want of critical evidence in its favour. Gorer was the first to provide that evidence, and to show that the immunological processes at work might not, after all, be so very far removed from those that provide for defence against infection. His scientific judgement was excellent: it must have been tempting for him to argue that the iso-antibodies he had discovered were themselves responsible for destroying the foreign growth, but he scrupulously refrained from doing so. Although in later years, as we shall see, he kept his mind open to the possibility that malignant tissue might be antigenically distinguishable from normal tissue, he made it quite clear that, so far as his own evidence went, there was no reason to doubt that the rules governing the transplantation of normal and of malignant tissues were essentially the same.

Gorer's work is of great importance in its own right; combined with the work of C. C. Little and his successors, it brought order and clarity into a field of research of which the results had been described, by one of its principal spokesman, as 'barren and inconsistent', and of 'protean diversity'. Yet, to Gorer's disappointment, his work attracted little attention. One reason is understandable enough; the surgical exigencies of the Second World War had not yet thrust the problems of grafting upon the attention of experimental biologists and pathologists. Another is less easy to excuse. The older practitioners of tumour transplantation were still preoccupied with the fads and fetishes into which empiricism ultimately degenerates, and they were still highly suspicious of genetical reasoning. Their failure to recognize the importance of Gorer's work revealed, at the very least, a lack of sensibility, and in retrospect it is hard to acquit them of lack of sense. As late as 1938 it was still necessary for Gorer to argue patiently in favour of controlled genetical experimentation and of the use of inbred, homozygous strains. He had an apt parallel: for cancer research workers to forswear the use of pure strains on the grounds that homozygous animals were abnormal was on all fours with a chemist's refusing to use pure compounds on the grounds that, in nature, chemicals are mixed.

Four years were to pass before Gorer wrote again on the transplantation of tumours. His earlier work had given him no reason to believe that the iso-agglutinins excited in *C57* mice by the inoculation of an *A*-strain tumour were responsible for the tumour's regression. Now, in 1942 (8), studying the transplantation of *A*-strain leukaemic cells, he devised a test which revealed the presence of protective antibodies: a small known number of leukaemic cells was mixed with an antiserum directed against them, and the mixture injected locally into *A*-strain mice. So treated, the cells very often failed to grow. This was the first demonstration of a protective antibody in tissue transplantation, though Thomas Lumsden's work beforehand—like his own, neglected—had shown that such antisera might have cytotoxic properties. Gorer picked up the threads of this work fourteen years later, in 1956.

Mention has been made so far only of Gorer's work on the immunological genetics of transplanted tumours, but from the outset of his career he was interested in the genetics of 'spontaneous' tumours as well. In one of his earliest publications (38) he expounded for the benefit of the readers of *The Lancet* the story of J. J. Bittner's famous 'milk factor'; in another (39) he wrote authoritatively against M. Slye's belief that susceptibility to cancer was inherited unifactorially; in a third (43) he reviewed evidence of the occurrence of tumours among human twins, calling attention to the safeguards that are necessary if evidence of this kind is to carry weight. Somewhat later on Gorer was invited by H. Grüneberg to share with C. C. Little the preparation of an appendix on 'The genetics of cancer' to the first edition of his standard text on *The genetics of the mouse* (42). Throughout his life, Gorer kept records of the occurrence of tumours among the inbred strains of his colony of mice, and reported upon them from time to time; his powers as a histopathologist are revealed by his careful descriptions of the morbid anatomy of renal lesions in elderly mice and his classification and analysis of the histiocytomas (40, 41, 48, 49). After the war, even when his work on transplantation absorbed almost all his energies, he found time to write a chapter (44) in Sorsby's *Clinical genetics* on the pitfalls in the diagnosis of tumours in human beings, particularly as they may affect the interpretation of vital statistics.

Let us now return to the Lister Institute and to the second of the two lines of research on which Gorer had embarked. (Later he was to call them 'sidelines', but they rank higher than that.) With H. Schütze (45, 46)—in one paper (45) with M. H. Finlayson as well—he described heritable variations in the resistance of mice to bacterial infection, finding that it was impossible even by active immunization to bridge the gap between animals of high and low natural resistance. Our knowledge of inherited differences in the responses of particular animals to particular antigens is still exiguous; Gorer regretted his inability to continue with the work, and some of the implications of F. M. Burnet's modern immunological theories made his thoughts return to it in later years. In 1940 (47), with D. W. Henderson, he demonstrated a synergism between the actions of antibody and of a sulphonamide drug (sulphapyridine, the 'M & B 693' of wartime) on *Vibrion septique*. The paper is noteworthy for its astute analysis—confirmed and extended by K. Mather—of the concept of synergism itself; he was to draw upon the concept of synergism once again, in work that was still in progress at his death.

In 1940 Gorer returned to Guy's Hospital. During the war his time was wholly occupied as haematologist and morbid anatomist, and his enjoyment of the work reconciled him to the almost complete abandonment of creative research. His paper on the formation of protective antibodies against leukaemic cells (8) was the first to issue from Guy's, but some of the work it described must have been done at the Lister Institute or at its outpost in Elstree, for at the end of the paper he thanks the Director of the Institute for 'enabling me to keep a number of the mice at Elstree during some of the worst bombardments'.

In 1942 Gorer married Gertrude Ernestine Kahle, the divorced wife of a German refugee of passionately liberal opinions who had fought with the Republicans in the Spanish Civil War. It was a happy marriage, but it ended in tragedy, for his wife died of tuberculosis on 8 May 1945—V.E. Day. There were no children.

The war's end was a critical stage in Gorer's career. The death of his wife had given him great distress, the quality of his work had not been widely recognized, and he must have felt himself out of touch with research. In retrospect it is easy enough to prescribe an almost ideal solution of his difficulties: to go abroad for at least a year to a laboratory superbly well equipped for work on the genetics of mice, and to collaborate with scientists expert in all matters to do with the transplantation of tissues. This solution was in fact arrived at. As a Research Fellow of the National Institutes of Health, Gorer spent more than a year over the period 1946-1947 at the one laboratory in the world that answered the above description, the Roscoe B. Jackson Memorial Laboratory at Bar Harbor, Maine (an institution Gorer had visited once before, briefly, in 1936). Nowadays such an arrangement would be easy to provide for; then, currency restrictions tied most Europeans to their homes, and it was therefore essential that Gorer should receive a stipend from American sources at a time when America had not yet expanded its programme of federal aid for research to the munificent proportions we have now grown accustomed to. His brother Geoffrey's help was decisive; it was he who took the initiative in arranging for Peter's sabbatical year at Bar Harbor, and smoothed his path financially.

At Bar Harbor, Gorer renewed his acquaintance, soon to become close friendship, with men whose work he had admired deeply for many years, chief among them C. C. Little, the Director, a grand master of the genetical theory of transplantation, and G. D. Snell, who had done more than any other man to translate that theory into a concrete special form. For his part, Gorer contributed his expert knowledge of pathology and immunology, his knowledge of antigenic variation in mice and the techniques that had revealed it.

The threefold combination of serology, transplantation and genetic analysis was immensely powerful. Three papers written with American colleagues represent the fruits of Gorer's work in the United States (9, 10, 37). One, published in the *Proceedings of the Royal Society* (10), demonstrates a close linkage between the gene corresponding to Antigen II and a gene, 'fused', affecting the bony conformation of the tail. (His colleagues had already revealed linkage between 'fused' and a gene governing the transplantability of *A*-strain tumours into mice of other strains.) And now the nomenclature of the genes governing transplantation were revised. At Snell's suggestion the genes were called *histocompatibility genes*, and the locus tenable by the allele associated with the formation of Antigen II became Histocompatibility-2 (*H-2*). Their work suggested that three alleles might be represented at this locus, and there were hints—later to harden into certainty—of the existence of several others.

Meanwhile Gorer was improving his serological methods. Haemagglutinating antibodies in mice are not very easy to reveal—some are 'incomplete', in the technical sense, and Gorer liked to classify them all as 'inconvenient'—but now it was possible for him to draw upon the empirical knowledge that had grown up around the detection of the Rhesus system of antibodies in human beings. At Bar Harbor, Gorer recognized three kinds of antibodies formed in *C57* mice by the inoculation of tumours indigenous to *A*-strain (9): his original saline agglutinins; agglutinins acting in the presence of 75 per cent mouse serum; and blocking antibodies, revealed by their power to inhibit the agglutination of red cells by agglutinating antibodies. He also believed he had evidence of a natural haemagglutin for *A*-strain cells in the serum of *C57* mice, but his later work did not uphold it.

Gorer's stay in Maine was to bring him personal happiness as well as a new lease of scientific life, for he fell in love with Dr Little's secretary, Elizabeth Bruce Keucher, and they were married at St Saviour's Church, Bar Harbor, on 3 June 1947. His married life was particularly happy, and in their beautiful home in Hampstead it formed a secure background to the rest of his scientific career. They had two children, Peter Charles, born 3 October 1948, and Rachel Gertrude, born 13 June 1950.

Shortly after their return to England, Gorer was appointed to a personal Readership at Guy's under Professor George Payling Wright; in spite of temptations to go elsewhere—his private means gave him the power to pick and choose—he remained in that position until his death, when plans were afoot for expanding and rehousing his unit and for creating a Chair of Immunology of which he was to be the first occupant.

From now on the story of Gorer's scientific research is one of logical progression and of steadily increasing cogency and analytical power. The style of his research changed, too, for now he was a senior worker and attracted pupils of great ability with whom much of his work was done. D. B. Amos, who has built up a considerable reputation for his serological work in America, was trained for his research career by Gorer; so were E. A. Boyse (who also went to America), P. O'Gorman and J. R. Batchelor. He spoke of all his pupils, and they of him, with pride and affection. Special mention should also be made of the devoted help he received from Mrs Z. B. Mikulska in the development and execution of his serological techniques. Dr John Merrill of the Peter Bent Brigham Hospital in Boston and Dr and Mrs E. Hoecker of the University of Chile learnt many of their special immunological methods under Gorer, and amongst others who came under his influence (though not so deeply as to enter into research on transplantation!) were Professor Alexander Bearn, the human geneticist, and Dr Humphrey Osman, a distinguished psychiatrist now in Canada.

By 1950 (12), following a lead from K. E. Boorman and B. E. Dodd, Gorer showed that antibodies which failed to agglutinate red cells in the presence of mouse serum might yet reveal themselves by conducting the agglutination test in the presence of heated human serum. In the presence of normal human serum, haemolysis occurred—the first demonstration of an isolysin in mice. Four years later (15), again following a lead from human serology, he and Mikulska found that the addition of a bacterial polysaccharide, dextran, to heated human serum could reveal iso-agglutinins of high titre with great consistency. This technique has come into use throughout the world. In 1956 (21), in collaboration with P. O'Gorman, he described the occurrence of cytotoxins in iso-antisera—antibodies which, in the presence of guinea pig complement, would destroy many kinds of normal and malignant cells *in vitro*. Leukaemic cells were particularly vulnerable; so also the cells of normal bone marrow and lymphoid tissues. With Boyse (26) he showed that sera containing these antibodies could prevent the establishment of grafts of bone marrow cells in irradiated mice—clear evidence that humoral antibodies might sometimes be the effectors of a 'homograft reaction' against normal as well as malignant cells.

We are indebted to Gorer not merely for the demonstration of iso-antibodies in mice, but for establishing the general proposition that the formation of iso-antibodies is a regular consequence of transplanting of homografts of all kinds. Gorer was particularly delighted by the discovery that haemagglutinating antibodies are formed after the transplantation of homografts of skin—even of 'pure' epidermal cells (13). This discovery was the outcome of collaboration between R. E. Billingham and E. M. Sparrow of University College and, in Guy's Hospital, of D. B. Amos, Z. B. Mikulska, and Gorer himself. In retrospect it may seem strange that the older and newer University College groups collaborated so little; but in fact, though there is no record of it, they kept in very close touch with each other and from time to time, when the nature of the work in progress made it seem a good idea, they met at dinner for the exchange of gossip and stop-press news. A Fellow of the Royal Society is asked to deposit in its archives a confidential document in which he writes about the influences that shaped his work. The author of the present memoir was very deeply moved to find that in this document Gorer should have made special mention of his 'close friendship with P. B. Medawar.' 'It is not easy to say exactly how each influenced the other's ideas,' he wrote, 'but the influence was none the less potent.' One of the ways we influenced each other was over the interpretation of the part played by humoral antibodies in homograft reactions. We at University College began by thinking that humoral antibodies were of secondary importance, and that the homograft reaction was mediated through the action of sensitized lymphoid cells, with the antibodies as lookers-on. Gorer, naturally enough, attached great importance to antibodies, and took some convincing of the importance of N. A. Mitchison's work on the cellular components of the reaction. In the outcome each recognized the importance of the other's ideas, and saw that a true explanation must certainly comprehend them both. At University College we became deeply interested in humoral antibodies, and drew freely on Gorer's techniques and ideas; Gorer for his part appealed in one of his later papers (28) to the concept of an 'allergic' death of sensitized cells when trying to explain the disappearance of *C57* cells after their inoculation into hybrid mice of the composition *C57*×*A*. It is significant that Gorer's last work, with J. R. Batchelor and E. A. Boyse, was on the synergism between humoral antibodies and sensitized lymphoid cells (33)—and this may turn out to embody almost literally the reconciliation we have both been seeking. Gorer's own opinion (20) was against a unitarian theory of the homograft reaction. Homograft reactions, he argued, were of at least three kinds: the reaction against normal or malignant lymphoid cells differed from the reaction against ascites tumour cells growing freely in the abdominal cavity, and that in turn differed from the reaction against solid grafts that established primary vascular and lymphatic connexions with their hosts. It was an error of judgement, then, to speak of '*the* homograft reaction'.

One of the paradoxes of a purely humoral interpretation of homograft reactions is the fact discovered by Gorer's old friend and colleague of Bar Harbor, Nathan Kaliss—the fact that under certain circumstances humoral antibodies can actually favour the growth or survival of transplanted cells. Gorer took advantage of Kaliss's sabbatical leave at Guy's to study the phenomenon ('enhancement') with its discoverer (29), finding that the growth of a certain sarcoma could be stimulated by low doses of an antiserum and inhibited by a higher. Gorer had always been fascinated by changes in the reactivity and vulnerability of malignant cells, particularly the changes that made a tumour more readily transplantable after a period of 'training' by temporary growth in foreign environments (7, 11, 16). As a geneticist he did not take kindly to the idea that greater ease of transplantation argued for the loss of antigens by mutation; special techniques, he pointed out, would often show up antigens that had been thought missing; a much more likely explanation was a reshuffling or redeployment of antigens represented at the surface of the cell.

The antibodies so far spoken of have all been *iso*-antibodies, formed by the exposure of an animal to antigenic substances taken from the tissues of other members of its own species. Yet for many years Gorer kept at the back of his mind the possibility that malignant tissues might have antigenic properties peculiar to themselves, and so give rise to antibodies that would react upon them but not upon normal tissues. The 'X factor' of Gorer and Amos may be an antibody of just this kind. The story—in due course to be studied in greater detail by Amos—was very remarkable (18, 19, 26): by means of a systemic injection of antiserum, it was possible to protect *C57* mice against the growth of a leukotic tumour (EL-4) indigenous to that very strain. The antibody, if such it was, could be absorbed by the cells of EL-4, but not by normal adult or foetal *C57* cells; the antigen it reacted with might be peculiar to EL-4, though certainly not to malignant cells in general.

While all this work was in progress, Gorer was slowly enlarging our knowledge of the structure of the *H-2* histocompatibility locus—the locus occupied by the allele responsible for the formation of his old original Antigen II. The first report (10) of his newer and deeper analysis of the *H-2* locus was published in the *Proceedings* of the Society, and so were its two successors of 1955 (17) and 1959 (27). The resolving power of Gorer's serological analysis had been improved beyond measure by his own developments of the technique of haemagglutination. The *H-2* locus is formidably complex in both the everyday and the technical senses of that word: by 1953, nine alleles or pseudo-alleles had been identified by the workers at Bar Harbor, and in 1959 the number had risen to twelve. By then, Gorer was able to make a tentative map of the region of the IXth chromosome in which the *H-2* locus lies. He was not satisfied that mutation had been clearly demonstrated, and he suspected that the combination *H-2a* as it occurs in *A*-strain mice may have arisen by crossing over between combinations *H-2d* and *H-2k*. The total number of pseudo-allelic combinants at the *H-2* locus, Gorer pointed out, was not yet known; other loci, though less important from the point of view of transplantation, might be equally complex. Indeed, the serological analysis of the known *H-2* combinations 'would demand the entire resources of a large institute for several years. . . . It is clear that detailed knowledge of the system will accumulate slowly and may never reach completion.'

It must be obvious from this account of his work that Gorer was much more than a man who made important particular discoveries. He was a master of the arts and theories of transplantation, a man whom everyone consulted and whose opinion everyone gave heed to. Guests came to his laboratory from all over the world, and he in turn travelled widely to give the lectures that were demanded of him—to New York often; and to Paris, Liège, the Netherlands, Italy (cp. 22 to 25, 31). Honours would have come his way automatically, though none perhaps that would have given him more pleasure than his election to the Fellowship of the Royal Society in 1960—an election that many of us thought overdue.

What kind of man was Gorer? He was tall and strongly built, though in later life he carried himself with a slight stoop; no one would be surprised to learn that he had been good at cricket as a boy ('to the detriment of my studies') but very few of his closest friends knew that he had represented England against Scotland and Belgium with the foil. He did not give the impression of being physically robust, and with a long history of asthma and bronchial disorders he could hardly have been so. His own special delight, as it had been his father's, was fishing, and he enjoyed it all the more for being a pretty competent naturalist—he had not allowed his childhood's interest to die away.

Gorer looked, as he was, extremely intelligent. The intelligence his face betrayed was not of the contemplative or inwardly reflective kind, but an intelligence ready mobilized—shrewd, quick on the uptake, instant in comment or rejoinder. Being generous-minded as well, it follows that he was an excellent conversationalist, and all the better for liking his own jokes as well as other people's. To call his face to mind is to think of him smiling, usually through a haze of tobacco smoke, for he smoked heavily during conversation and argument. His conversation was quick, racy and elliptical; he set a good pace and inspired others to keep up with it. When he delivered scientific papers he sometimes overestimated his audience's knowledge and speed of comprehension, an implied compliment that left them flattered but sometimes a little dazed. Gorer had a real sense of language. His papers are short, clear and to the point; they are light in texture even when they are most concentrated in thought. In his earlier writing one can see Haldane's influence, and an expert might detect his elder brother's; but later his style took a shape distinctively his own. One of Gorer's last works was to try to reform the grotesque, almost cabalistic terminology by which students of transplantation make their ideas known to one another (32, 34). The substitution of 'allogeneic' and 'syngeneic' for 'homologous' and 'isologous' is a wholesome reform, and one can but hope that the new words stick.

Gorer was a man of complete intellectual honesty. This made it possible for him to criticize others without fear of giving offence, and for others to contradict him flatly without a thought of offending him. His appraisal of other people's work was sometimes peremptory. He once misjudged a doctorate thesis, frankly disbelieving the evidence put before him; but it did not take him long to become convinced of its merits and he soon became good friends with its author. He could write scathingly (as he did once of a certain geneticist's assertative opinions on the subject of malignant growth) but only when he smelt out high pretensions not backed up by knowledge or judgement. Of honest work or opinion he never spoke unkindly. Gorer was held in great affection by his scientific colleagues and his many other friends; the sorrow and dismay caused by his death was for the loss of him personally, not only for the loss to science that his death entailed.

Peter Gorer died peacefully in his sleep of an oedema of the lung, on 11 May 1961, about three weeks after the diagnosis (wisely and skilfully withheld from him) of cancer of the lung. What he did assures him of an honoured place in the history of the branch of science he gave his mind to; what he could yet have done gives him the special place that is reserved for those who die with their work unfinished, still on the high roads of scientific discovery.

P. B. MEDAWAR

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Ross G. Harrison

ROSS GRANVILLE HARRISON

1870-1959

The published research of Ross Harrison, the inventor of tissue culture, and amongst the greatest of experimental embryologists, spans a period of 54 years. Before describing this immensely important contribution to biological knowledge, which must be the main concern of this memoir, the biographical framework in which it was made needs first to be sketched.

Harrison was born on 13 January 1870, in Germantown (Philadelphia), Pennsylvania. His father, Samuel Harrison, was an engineer whose work took him for long periods abroad. His mother died when he was a child. After schooling in Baltimore, he entered Johns Hopkins University in 1886, somewhat undecided about his future. He recalls, in his Croonian Lecture, that it was Newell Martin, T. H. Huxley's assistant and one of the original group of professors at Johns Hopkins, who first inspired him to become a zoologist. He started graduate work with W. K. Brooks, Martin's successor, in 1889, amongst his fellow-students being two of his great contemporaries, E. G. Conklin and T. H. Morgan. He went abroad to work with Nussbaum in Bonn in 1892, establishing a connexion with German anatomists which was of great importance in his development. His Ph.D. at Hopkins followed in 1894, and after a year as Lecturer in Morphology at Bryn Mawr College, where T. H. Morgan had become Associate Professor of Biology, and another visit to Bonn, in 1896 he joined the staff of F. P. Mall, Professor of Anatomy at Johns Hopkins and a distinguished embryologist. In 1896 he married Ida Lange, whom he had met on his first visit to Bonn. They had a family of three daughters and two sons. He returned briefly to Bonn twice before the end of the century, and took his M.D. there in 1899, the same year that he became Associate Professor of Anatomy at Johns Hopkins. He stayed a further eight years in Baltimore, and this was the period of his momentous invention of tissue culture. The brilliance of this research is all the more astonishing in that he was at this same time launching and guiding, as managing editor, the new *Journal of Experimental Zoology*; and as he later ruefully remarked, in those days the editor of a scientific journal had to be business manager and office boy as well.

In 1907 he left Hopkins for Yale, where he was Bronson Professor of Comparative Anatomy until 1927 and Sterling Professor of Biology from 1927 to 1938. He was for most of this time Chairman of the Department of Zoology, and he played a large part in designing and supervising the construction of the Osborn Zoological Laboratory, of which he became Director. He retired in 1938, though without severing his connexion with his laboratory, where he worked to the end of his time. He became Chairman of the National Research Council in 1938, and held the office until 1946, playing a great part in scientific administration during this crucial period. He died on 30 September 1959.

Harrison's early papers, published in German between 1893 and 1895, were descriptive studies of fin development in the salmon. Nussbaum, during Harrison's first visit to Bonn, apparently started him in this line of work, but he carried it on by himself when he returned to the United States in 1893. It became the subject of his Ph.D. thesis. Harrison may be said to have emerged fully-fledged as a descriptive embryologist. He showed a marked talent for exact and significant observation, and he had a precise and methodical style of exposition. His sure grasp of his own immensely detailed results, and of the published work of others, was already evident. These long papers were a fine achievement for a young man, still under 25; and in its main outlines the work still stands.

His Ph.D. behind him, Harrison made his brief excursion to Bryn Mawr, and then returned to Mall's department at Johns Hopkins. With this change of circumstance, new interests in research emerged. His next two major papers turn to fields that foreshadow his future preoccupation with experiments on the embryology of the nervous system. The first of these papers marks his initiation into experimental zoology, the second into developmental neurology.

The experimental paper (1898) seems to have been more an expression of enthusiasm and pleasure at mastering a remarkable new technique than a major contribution to science. A short time before, Born had found that it was possible to perform highly radical grafting operations between frog embryos, even between those of different species. The next forty years of experimental embryology was to be dominated by this technique and its derivatives. Harrison was evidently enchanted by the possibility of making frogs with the front half of one species and colour, the back half of another. He found, of course, some new observations to make, but the paper was mainly justified in that it introduced the technique to the frogs of North America. This form of flattery of the inventor of a new method was something that was to happen on a much bigger scale to Harrison when he invented tissue culture.

The second paper (1901) was an exceedingly thorough description of the early developmental relations of the peripheral nervous system to the neural tube in the salmon. It plunged him immediately into one of the great controversies of his time, as to how the nerve fibre developed. Three main theories had been struggling for supremacy for many years. In outline these were: (1) the theory associated with Schwann and Balfour, that the fibres were secreted by the chains of sheath cells which normally surround them, the Schwann cells of the peripheral nervous system and the glial cells of the central nervous system; (2) the theory of Hensen, which asserted that the cells of the body are from the earliest stages intricately connected together by fine protoplasmic threads, and that nerve fibres differentiate by the specialization of certain of these threads, the selection of which should differentiate taking place under the influence of function; (3) the theory of His and Cajal, according to which all nerve fibres grow out from neurones situated in the central nervous system or in the ganglia. Hensen's theory was probably in the ascendant at the turn of the century, and its most important competitor was the His–Cajal outgrowth theory. In the end it was Harrison who decided the conflict.

The significance of this controversy between the Hensen and His–Cajal theories did not, perhaps, lie only in the decision as to the basic mechanism by which connexions in the nervous system are formed, fundamentally important as that is. As so often with such hotly contested disputes, it seems to have symbolized more. It was a struggle round the entire cell theory, a struggle between those who felt that the mystery of the organism as a whole is utterly lost to sight when it is subjected to analysis, and those whose approach is atomistic, who try to resolve the whole into parts from the interrelations of which the whole can be derived. But this is oversimplified, since the Hensen theory had undoubtedly a more rational appeal: for it provided a deceptively simple explanation of how the nervous system makes the right connexions, while it remains to this day a puzzle how it does so according to the His–Cajal theory.

It is interesting that Harrison, a model of objectivity in all important respects, in this his first paper on the subject gives not the faintest sign of a judicious weighing of evidence and balancing of opinion concerning the rival theories. He is perfectly clear from the start that the His–Cajal theory is right. Perhaps the observations of himself and others gave him no rational alternative, but this seems hardly consistent with his string of subsequent papers all trying to prove the same point. Perhaps Mall, who had actually worked with His, influenced him. Perhaps his analytical cast of mind preferred the cell theory in its starkest form. Whatever it was, one suspects that Harrison, for all his objectivity in obtaining and assessing evidence, worked on strongly held hunches at least until the evidence was available. We shall come across later indications of this trait, which is, of course, far from uncommon amongst great scientists.

After this account of the origins of the peripheral nervous system, Harrison published no more purely descriptive papers. This is not to say that he published no more descriptive work: he published much, but always as an essential preface to a set of experiments. Perhaps a remark he made ten years later is significant: 'to fill in the details of descriptive anatomy, histology and embryology is not a sufficient task to keep one's wits sharpened'.

Before proceeding with his experimental attack on the nerve fibre problem, he published in 1903 another experimental paper which must be mentioned, though it is rather isolated from the main stream of his work, because it has become a classic. In it he analyzed the extraordinary extension of the material which forms the lateral line sense organ of the amphibian larva, from a localized placode in the head to several long strands, one of which runs backwards as far as the tip of the tail. The work arose directly from his previous use of Born's technique: a chimaeric animal, the head end of which came from a dark species, the hind end from a pale one, developed a dark lateral line along the tail. By means of many skilful variations of the grafting, he showed that the lateral line material had a preferred pathway for its extension along the side of the embryo, and that its power of movement was autonomous. The work was linked too with his favourite subject, because he found evidence that the innervation of the lateral line placode occurred by outgrowth of nerve fibres from neurones in the vagus ganglion.

It was evident that no rational decision between the theories of nerve fibre origin could be obtained without experiment. Hitherto, all sides had used as evidence preparations of the normally developing nervous system, commonly of exquisite beauty and, whatever the theory they supported, of remarkably similar appearance. The general trend seemed to be, however, in Hensen's favour. In the next few years, Harrison was occupied with the experimental attack. He mounted a rather incoherent onslaught on the Schwann and the Hensen theories, which reads as if he was determined to throw everything he had at them, which was a good deal. The onslaught was all the more required, because Braus and Banchi, feeling too that only experiment could give the answer, had done the experiments and decided in favour of the wrong theory. Harrison at least restored the situation with a specific counterblast to these two which demonstrated that, even if their observations were right, their conclusions did not necessarily follow, and in any case their observations were wrong.

The points that Harrison, in half a dozen publications, succeeded in establishing were as follows: (1) Complete removal of the central nervous system eliminates nerve fibres, even though the cell bridges postulated by Hensen were, presumably, undisturbed peripherally. He excised the spinal cord or whole neural plate, rearing nerveless larvae by the ingenious trick of uniting them in parabiosis with normal larvae. He also isolated, before its innervation, a limb-bud from the central nervous system by means of an obstruction, and observed that the limb was nerveless. These experiments, incidentally, also served to show that the general form of the muscular system developed independently of nerve supply. (2) When nerve cells are present, but in abnormal surroundings, nerve fibres nevertheless develop in connexion with them. He transplanted fragments of cord into nerveless embryos, and nerve fibres appeared connected with the grafts. More important, he showed that such nerve fibres would develop even when any of Hensen's intercellular bridges which might have originally been present had been cleared away: nerve fibres grew from the brain into connective tissue which had replaced the excised spinal cord, and, a stride towards tissue culture, into cylinders of blood clot that he put instead of the spinal cord.

(3) Nerve fibres could actually be observed to extend progressively towards the periphery, in severed nerves in the tadpole tail fin. This important technique of prolonged observation of living tissue, exploited later by Speidel in some notable papers, was apparently due in the first place to Harrison. (4) Function is not necessary for nervous development. Partly the evidence was derived from the nerve fibres that appeared in the experiments mentioned under (2) (Warren Lewis had done similar experiments showing the development of olfactory fibres, functionless in the sense under discussion, into a space from which the brain had been removed); partly the evidence came from an experiment, often quoted since, in which embryos were reared for a week or more under chloretone anaesthesia, and when they came round had the behaviour patterns, and neuro-muscular histology, appropriate to their age ready developed. This undermined Hensen's attractive subsidiary hypothesis about how appropriate neural connexions were made. (5) Axons appear in the absence of Schwann cells. He was able to eliminate the latter experimentally by removing the dorsal part of the spinal cord or brain, including the neural crest, and naked motor nerve fibres then appeared; and he was able, too, to point to naked sensory axons in normal development. (6) Schwann cells do not form nerve fibres when the appropriate neurones are eliminated. He eliminated those of motor nerves by removing the ventral half of the spinal cord, leaving the source of Schwann cells intact. He also grafted normal limbs to nerveless larvae, and found they completely lost their axons.

The conclusions from this body of work were then that, for the formation of axons, the original intercellular bridges are not necessary, function is not necessary, Schwann cells are not necessary, but neurones are necessary. Nevertheless, the destruction done on the theories other than that of His and Cajal did not conclusively demonstrate the truth of the latter. It was still possible to suppose that an influence flowing from the neurones was required to convert cell bridges into nerve fibres; and that cell bridges could be re-formed by other cells if they had been destroyed, for in Harrison's experiments new axons always appeared in areas already containing other cells. Furthermore, such an attempt to save the Hensen theory could well be justified, because Harrison had so far produced no positive evidence that nerve fibres ever actually did grow out of neurones. Strong as the summation of his evidence was against Hensen and against Schwann, the simple direct demonstration that the His–Cajal hypothesis *could* work was lacking. If anything the grip of Hensen's theory seems to have been extending during the first decade of the century in spite of this experimental attack. Harrison, therefore, proceeded to invent tissue culture, thereby clinching his argument and making his most influential contribution to zoology.

The technique was a natural extension of his work with blood clots put into the larva; just as Leo Loeb had evidently tried the technique about ten years earlier to follow up experiments with blood clot and agar containing epithelium implanted in an adult mammal. But Loeb apparently had been unable to make the method work, perhaps because he used agar. The idea of cultivating isolated cells outside the organism was also in the air at the time. Haberlandt, who saw its importance clearly, had recently attempted it with plant cells, and there had been some successful cultures of blood and lymph cells. But Harrison's explanation of tissue fragments was something new.

Harrison isolated fragments of neural tissue from a frog embryo in drops of fluid hanging on the under-surface of a coverslip inverted over a hollow-ground slide. His explants died in simple sodium chloride solution or in Locke's saline, without any visible outgrowth of axons or other cells. This failure of outgrowth was probably not due to any defect in the saline, but was because he had not then realized the crucial necessity of a solid substrate for cell movement. He quickly drew the right conclusion from this experiment, and from others made during his preliminary search for a cell-free space in which he had made implantations into the ventricles of the brain and into the pharynx of embryos. The conclusion would not have been difficult to draw since Loeb had already emphasized the importance of solid surfaces; and Harrison already suspected that the role of intercellular bridges and other thread-like structures in the embryo, insofar as they were not fixation artifacts, was to act as a guide to growing nerve fibres. A medium providing solid supports was indicated. Gelatin was unsuccessful. Lymph from adult frogs, which has a high fibrinogen content and readily clots to form fibres similar in size to the much-discussed intercellular bridges, solved the problem. Meanwhile, he was naturally having trouble with infection, but he finally overcame it with repeated washing of the embryo donors with sterile distilled water, and strict asepsis in all subsequent manipulations. In the spring of 1907, in the Anatomy Department of Johns Hopkins, he made the first successful cultures of tissues and opened a new era. A brief note appeared in the *Proceedings of the Society for Experimental Biology and Medicine* in May 1907. He continued the work when he moved to Yale in that year, and in 1910 published his classic paper 'The outgrowth of the nerve fibre as a mode of protoplasmic movement'.

The results he obtained from these cultures were simple. Long cytoplasmic threads extended out into the cell-free medium from neurones in his explants, and could be watched doing so. The tips of these threads showed active pseudopodial movement, as Cajal had suggested many years before that they should. They closely resembled the axons found in the embryo. No other tissue he tried, except those known to form neurones, produced similar threads, though they had outgrowths of other kinds. It was a few years after the prelimnary paper before Montrose Burrows showed that these threads in cultures of nervous tissue had the classical staining properties of nerve fibres, but Harrison's point was already effectively made.

These two publications of Harrison's are an astonishing stride forward in the history of biology. He simultaneously unleashed a new technique of tremendous power, and solved a hotly contested problem. Now that it was positively shown that nerve fibres could be formed by outgrowth from a neurone, all the other evidence clicked into place. The origin of nerve fibres in general was thenceforward decided for all biologists who were not hopelessly committed to the other theories, hopelessly anti-experimental, hopelessly out of touch with the publications of others, or all three. Confirmation of the outgrowth of nerve fibres *in vitro* quickly followed from Burrows, and with delightful ferocity towards unbelievers, from Warren and Margaret Lewis.

As if carried away by excitement, Harrison allowed himself in his 1910 paper a luxury rare for him, a long and rather speculative discussion (18 pages), far beyond the tolerance of a modern editor. It is a revelation of the far-ranging soundness of his thinking. It contains a whole series of fruitful suggestions, and, more extraordinary, no unfruitful ones. He introduced the idea of pioneering nerve-fibres or 'pathfinders' along which later-growing fibres are guided. He made perspicacious remarks about the difficulty of deciding whether cells are in syncytial junction or not (he had shown experimentally that the living neural plate, which looked syncytial, could readily be dissociated into separate cells). He suggested that the conformation of the medium in which cells lie or move profoundly affects their external shape. He speculated as to whether the specificity of the connexion between nerve fibre and end organ is comparable with the specificity of sperm and egg. He emphasized the role of cell movement throughout morphogenesis, and asserted that this movement has its seat in the hyaline ectoplasm at the borders and angles of cells. He clearly recognized the immense potential scope of this new technique of his which allowed living observation of cells in controlled conditions.

Harrison's papers on tissue culture did not have to wait long before the exciting possibilities they opened up were recognized. The achievement, indeed, had much publicity. Montrose Burrows came from the Rockefeller Institute to work with Harrison, and showed that the chick embryo would serve as well as the frog. With that the gold rush (as Harrison was fond of describing similar episodes in recent biological history) was on. There could be no greater testimony to the enthusiasm aroused than the way that highly distinguished research workers like Warren Lewis and Alexis Carrel dropped everything to become thenceforward tissue culturists. Not so, however, Ross Harrison himself. He had invented the technique, for a specific purpose in neurology. It had solved that problem, but it evidently did not seem to him a convenient way of solving any of the other problems he had in mind. For him problems came first, and his were pre-eminently morphogenetic ones. He perhaps revealed his feelings when in a later retrospect (1928), though with typical tolerance commending the devotion of so many tissue culturists to the preparation and characterization of pure cell lines, he observed that it was as if chemists studied only pure substances and left out their inter-actions. Morphogenesis is inseparable from cellular interaction, and it was here that Harrison's interests lay.

But Harrison did publish a little more on tissue culture. He was clearly much concerned with locomotory behaviour of cells, specifically with how moving nerve fibres establish the complex but reproducible patterns of the nervous system, though he did not find a useful way into the problem. In later papers he drove home the point he had had to learn in devising the technique, which is also a point of the first importance for the study of cell behaviour, that their movement requires a solid substrate: he called this the 'positive stereotropism' of cells. He demonstrated it for mesenchyme of both frog and chick, using a variety of substrates, including spider web. He noted that when the solid substrate 'has a specific linear arrangement, as in spider web, it has an action in influencing the direction of movement as well as upon the form and arrangement of cells' and he thought that such an action might occur normally in the embryo, a suggestion fruitfully followed up by Paul Weiss some years later. Harrison's Croonian Lecture of 1933 is a full review of the problems he was concerned with during this neurological period, and long continued thinking about afterwards: the wanderings of nerve fibres and how they are directed.

Though Harrison's work appealed at once to experimental embryologists, there was plenty of strong feeling, especially in his own profession of anatomy, that all experiment was so basically abnormal (as compared with the entire normal study of violently precipitated embryos cut into thin slices) that it could not be taken seriously. Tissue culture was an extreme challenge to such traditionalists, and Harrison was moved to produce what is almost an experimentalists' manifesto in his address as President of the American Association of Anatomists in 1912. It is a brilliantly perspicacious lecture on the scope and methods of anatomy, dated only insofar as a few of the things he advocates have by now been done. It assesses the achievement and the future of the comparative-descriptive and of the experimental methods, and pleads for the freedom of anatomy from 'the domination of the concept of the organism as a whole'. 'The experimental methods of study will enable us to state the facts of morphogenesis in simple terms of cellular activity, and we may hope to connect these with the results of microchemistry in localizing intracellular activities, and ultimately identify problems of structure and function with those of the protein molecule in its manifold physicochemical relations.' Yet he ends with a plea for tolerance of *any* sort of research that will arouse the interest and enthusiasm of the investigator even though the importance of a fact to science is often of quite a different order from its importance to its discoverer.

Though to all reasonable people he must seem to have closed a chapter in the study of the development of the nerve fibre, he published some more on this theme. A long paper in 1924 reconsidered all the evidence against the Schwann–Balfour theory, since Harrison apparently felt that he had been too preoccupied with Hensen's theory to bury his other opponents with full honours. There can surely have been few still available for conversion to the outgrowth theory; but Harrison's standards of evidence were not those of the world in general. In addition his early experiments, here expanded and published in full, contained some fundamental information about the neural crest, being in fact the first experimental work on dorsal root ganglion and Schwann cell origins. Nor was this his only contribution to knowledge of this extraordinary tissue. Amongst other important points, he was almost the first to suggest (in his tissue culture paper of 1910) that pigment cells were derived from neural crest. Much later, in 1937, he gave a masterly review of the neural crest when presiding at a meeting of the Anatomische Gesellschaft.

But after the invention of tissue culture Harrison was moving on to other interests. Two main lines of research occupied him for the rest of his life: polarity and symmetry in embryonic organs, and the control of organ growth. Both pose deeply important problems, both drew from him some highly original work, neither have greatly advanced since him.

Harrison's work on polarity and symmetry began with a study of the fore-limb-forming area of early amphibian embryos. The first of a series of papers appeared in 1915. He showed that this area of the embryo's flank contained the potency to form a fore-limb, independent of its position in the rest of the embryo. But within the area there was no localization of potency to form specific parts of a limb: the area is an equipotential system in the classical sense, in that any half of it will give rise to a whole limb, and two areas superimposed in concordant orientation will form a single limb. This power of 'regulation' was already well known for a number of embryos at cleavage stages, and Braus in 1909 had suggested that the rudiment of the shoulder girdle is an equipotential system; but it is to Harrison that we owe our current picture of the later vertebrate embryo as proceeding to organ formation through such regulatory 'fields', each with a general specification to form an organ, but only later breaking up into sub-areas specified to form particular parts of the organ. Harrison found, however, that something else was firmly specified within the equipotential fore-limb areas: the antero-posterior polarity, the decision as to which side would become pre-axial, which post-axial. In his great paper 'On relations of symmetry in transplanted limbs', which appeared in the *Journal of Experimental Zoology* of 1921, he reported the results of systematic transplantations of the fore-limb area so that its antero-posterior and dorso-ventral axes were concordant with or opposite to the corresponding axes of the host's body. By moving a right limb area to the left side of the body he could alter the concordance of the antero-posterior axis without altering that of the dorso-ventral axis, and vice versa. The analysis of the results is much complicated by the tendency of the transplant, in certain orientations, to duplicate as it develops, and the symmetry relations of these reduplicated limbs had to be disentangled before the basic behaviour could be determined. The basic behaviour, however, was simple. The antero-posterior polarity of the limb area cannot be altered by unconformity with that of the host at any stage tried. The dorso-ventral polarity is on the contrary converted to that of the host up to a certain stage; but it too then becomes fixed. The limb area at about this time ceases to be an equipotential system.

In interpreting the nature of this quite unexpected polarity, Harrison seems to have had from the start a strongly held hunch, derived from Driesch. He thought that the polarity must reside in a polarization of elements—and clearly from very early on he had in mind protein molecules—distributed throughout the area. Because of C. M. Child's powerful influence, a different theory had much more immediate appeal: that there is a single gradient of some property from one side of the area to another. But Harrison merely gives this theory an uninterested glance. As a matter of fact, he later (1925) got evidence which would be very difficult to reconcile with a gradient theory: normal limb formation after exchange of the anterior and posterior halves within a single limb region. But he does not seem to have troubled to use it to support his own interpretation.

The paper is unsurpassed in care, clarity and grasp. It is simply definitive. But it did not, like his tissue culture paper, or like Spemann's organizer work which was coming to fruition about this time, open up an exciting vista of experiments to be done. There was no gold rush. Harrison followed up the work himself in his usual methodical way by repeating it on other embryonic systems. He analyzed the ectodermal ear placode, and found a substantially similar state of affairs, except that he could here detect an initial isotropic state preceding the fixing of the antero-posterior polarity. It is most interesting that, as members of his school showed, this antero-posterior polarization coincides with the establishment of the direction of the epidermal ciliary beat (Twitty) and of the direction of extension of the lateral line placode (Stone). When discordant transplantations are made after the antero-posterior polarization has occurred, there is commonly partial doubling of the ear along this axis, the two parts being perfect mirror-images of each other. This recalls the similar, but more extensive, reduplication of limbs transplanted at a corresponding stage. Harrison regarded the phenomenon as perhaps the best evidence there is for the paracrystalline nature of living matter. There is a curious phase as the later dorso-ventral polarity of the ear is being determined when, if the placode is grafted upside down, its whole development is badly upset without apparently its dorso-ventral polarity being reversed. This work on the ear rudiment, which included incidentally important observations on the initial induction of the ear placode, was never gathered into one of his grand papers, but it was nevertheless a beautifully executed programme involving some 700 specimens. He also did many experiments on the external gills from this point of view, but the system evidently proved to be too complex to be worth a detailed analysis. It led on, however, to a very full descriptive and experimental study of the development of the balancer in *Amblystoma* (1925).

Obviously, though, Harrison wanted to probe deeper into his idea about the nature of the polarities, and he was led to a daring shot in the dark. During 1937, he came to work with Astbury in the Department of Textile Physics at Leeds, in the hope that X-ray diffraction analysis of living embryonic structures would show up the orientated molecules he postulated. They worked out the necessary technique, but got no positive results, entirely excusably in view of the complex nature of the material. Nobody else seems yet to have thought of a way of tackling this intriguing problem of intrinsic polarity.

His work on growth arose out of transplantation of limbs between amphibian species of different size and growth rate. He found (1924) that grafts of limb-buds from embryos of the large species, *Amblystoma tigrinum*, placed on host embryos of the small species, *A. punctatum*, grow much bigger than the host limbs, and even surpass the donor limbs. Conversely, *punctatum* limb-buds grafted to *tigrinum* grow much smaller than the host limbs and smaller than the donor limbs too. He proposed, therefore, that growth of the developing limb depended in the first place on an intrinsic growth potential (later shown to reside in the limb mesoderm, not the ectoderm) which was higher in *tigrinum* than in *punctatum*; and in the second place on a circulating growth regulator which on the contrary was at a higher level in *punctatum* than in *tigrinum*. He followed a hunch that the growth regulator was hormonal, probably from pituitary or thyroid, and this time, most unusually, was led astray. A series of experiments to test the hormone theory, including the exchange of the hypophyseal rudiments between embryos of the two species, came to nothing; and two members of his own school, Twitty and Schwind, then showed that the effect of the supposed regulator could be the expression of a relative malnutrition of the large and voracious *tigrinum* larvae.

Similar results were obtained from interspecific grafts of ear and gill, but the most complete analysis, which was written up in a major paper, concerned the eye. These experiments, while once more demonstrating the autonomy of the intrinsic growth potentials in the different species, showed that the growth of the developing eye could be much influenced by its own internal interactions. The lens of a small species in the eye cup of a larger species, for instance, grew unusually large; but the eye cup was simultaneousy reduced in growth rate, so that the two components came to match each other at, so to speak, an intermediate size.

Harrison's second Harvey Lecture, published in 1935 (his first, on the development of the nervous system, had been twenty-five years earlier), reviews the work that he and his pupils did on these problems, and incidentally reveals a distinct evolution of Harrison's thinking towards a more sophisticated quantitative approach than he had hitherto felt necessary. Twitty subsequently added some important further results on grafts between animals of different ages. The whole body of work, though it has revealed more problems than solutions, is probably the most extensive experimental attack on the growth of animal organs yet made, based on a characteristically simple and original idea of Harrison's.

When one attempts some sort of summary and assessment of Ross Harrison's achievement, one feels no hesitation about regarding his list of discoveries as hugely impressive. One might be tempted to regret, perhaps, the great elaboration with which he sometimes established rather small points, his tendency 'to work over his subject so exhaustively and perfectly that no contemporary is able to improve upon it' as he himself said about another great biologist of his time, E. B. Wilson. One feels that others could hammer away, but Harrison had much more unusual and important abilities to employ. But perfectionism was part of his style of work, and seeing what strength it contributed to his research there is no sense in regretting its occasional over-indulgence.

In his contribution to the theoretical structure of biology, Harrison was hardly in the same class as his fellow graduate student at Johns Hopkins, T. H. Morgan; but his subject did not permit him to be. As he said, the difficulties of embryology 'have made its practitioners go ahead wherever an opening seemed to appear, with little thought to building a comprehensive and internally consistent theory'. Harrison did the same, except that it is unlikely that he gave it little thought. Seen from this distance, though, it is improbable that any other embryologist, including Spemann, contributed more to theory. The state of knowledge was inadequate for minds even as great as these to make far-reaching syntheses.

It is possible, however, to make an immensely important contribution to thinking which is not embodied in formal theory, by creating an attitude of mind, a subtle influence on the kind of hypothesis proposed and experiment done. It is difficult to assess such a contribution, but at least it can be said that Harrison was the central figure in the shift away from the mysteries of the organism as a whole, which have always been so tempting in morphogenesis, and towards analysis. His own work directly, and the huge movement which followed his invention of tissue culture, threw the emphasis on cells as units of interaction, and he pointed beyond that to the consideration of cell behaviour in terms of molecular structure. Modern cell biology in its developmental aspects owes a debt to Harrison greater than to any other individual for shaping its whole mode of thinking.

Finally, we must record the stimulation of productive work in others that Harrison brought about. Through his invention of tissue culture it was world-wide. In some ways it seems now regrettable that he never took over the leadership of tissue culture himself. From the moment of discovery he saw the pitfalls and pathways ahead with a clarity that the tissue culture movement as a whole did not attain for another thirty years. One finds, for instance, in 1912 such a remark as 'in respect to the phenomena of cell multiplication and growth . . . unfortunately we here have a difficulty in distinguishing between mere movement (wandering of cells) and actual growth due to proliferation'. How much misplaced research could have been saved if this simple caution had been drummed home with Harrison's authority. But no doubt Harrison was right in turning to other fields, where his part could be a more constructive one.

He stimulated valuable work not only through his papers but very notably through his many distinguished pupils. He held to a sink-or-swim policy in training research workers, like Brooks who trained him, and Mall his first chief. He found some remarkably good swimmers such as Burns, Detwiler, Hooker, Nicholas, Stone, Swett and Twitty, and one suspects his role must have been a great deal more than merely throwing the young research worker in.

Ross Harrison indeed made an irresistible impression. His scientific strength was tremendous: mastery of technique, great knowledge held in just perspective, powers of penetrating questioning and objective answering over so wide a field, patience and persistence to match his exacting standards of evidence, and imaginative resource of the first order. Yet he was a modest and gentle person. He certainly did not shrink from controversy, but nowhere in his writings is there an unfair or discourteous word. In 1912 Leo Loeb published a paper on tissue culture in which he made no reference to Harrison, and claimed for himself the invention of the technique. This unhappy situation was met with no counter-claim by Harrison, but with expressions of respect, perfectly sincere because well-merited, for Loeb's work. No wonder that Ross Harrison captured the devotion of so many.

Harrison's great services to science in ways other than research must receive quite inadequate treatment in this memoir. Much of his time was consumed in administration, surely less congenial to him than laboratory work, and done from a strong sense of duty. Scientists in the United States, and indeed everywhere, owe him a lot for these little-recorded activities. His work for the *Journal of Experimental Zoology*, of which he was managing editor from its beginning in 1904 until 1946, has already been mentioned. This journal, started by a small group of enthusiasts, was for some time actually published from Harrison's rooms in the Anatomy Department of Johns Hopkins. Such was the talent ready to be stimulated and focused by the new channel of publication, and such was the editor, that it immediately became one of the world's great biological journals. Not only did Harrison look after 103 volumes of the *J.E.Z.*, he served on several other editorial boards. Throughout his life he was active, too, in the secretaryships, committee-memberships and chairmanships that oil the wheels of scientific work when such men hold them. He gave his services for many years to, amongst numerous other institutions, the Marine Biological Laboratory and the Oceanographic Institution at Woods Hole, the Wistar Institute, the Rockefeller Institute, the Bermuda Biological Laboratory and the Long Island Biological Laboratory; and, amongst numerous scientific societies, the American Society of Zoologists, the National Academy of Sciences, the American Philosophical Society and the Anatomische Gesellschaft. His public service culminated in his Chairmanship of the National Research Council from 1938 to 1946.

Harrison received honours from the scientific world too numerous to detail. He delivered the Croonian Lecture to the Royal Society in 1933 and was elected to Foreign Membership in 1940. He was similarly honoured by the Scientific Academies of Norway, Sweden and the Netherlands, by the Accademia dei Lincei, and the Académie des Sciences of the Institut de

France, and by many other distinguished scientific societies in Europe and America. He received honorary degrees from many universities of the United States, and from Freiburg (Spemann's university), Trinity College Dublin, Budapest and Tübingen. His laboratory at Yale will be kept as a memorial to him.

I should like to record my gratitude to Miss Sally Wilens for the photograph of Dr Harrison, for valuable biographical information, and for the list of papers that follows.

M. ABERCROMBIE

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Geoffrey Jefferson.

GEOFFREY JEFFERSON

1886-1961

LIFE

GEOFFREY JEFFERSON was born in County Durham on 10 April 1886, the son of Dr A. J. Jefferson, a well-known surgeon and general practitioner in Rochdale, Lancashire. His great-grandfather had served in the Royal Navy, being entered as midshipman to Captain Bligh, later of *Bounty*.

He was educated at Manchester Grammar School, and thereafter in the Faculty of Medicine of the University of Manchester. He graduated M.B., B.S.(London) in 1909, proceeding to the Fellowship of the Royal College of Surgeons in 1911 and to the M.S.(London) with gold medal in 1913. For a time he was demonstrator of anatomy to Professor Elliott Smith. While in the last-named post, Jefferson made some contributions to the study of the cerebral sulci, and it was this introduction to the nervous system that stimulated his lifelong interest in this system, as anatomist, physiologist and surgeon.

In 1914, Jefferson, having married a former fellow-student, Dr Gertrude Flumerfelt, herself to become a psychiatric physician of parts, decided to move to Vancouver, British Columbia, his wife's childhood home, in the hope of opportunities of a surgical career.

The pathway to opportunity, then, and still largely today, for an able and ambitious young physician or surgeon was appointment to the staff of an undergraduate teaching hospital. Denied it by lack of vacancies at the relevant time, young medical graduates, now as then, migrate across the Atlantic and promising men are lost to us.

But the first world war broke out shortly after the Jeffersons' departure, and they speedily returned, Jefferson to join the staff of the Anglo-Russian Hospital organized by Sir Herbert Waterhouse. After some service in Russia, Jefferson went to France and with the 14th General Hospital began, under the hard conditions of the war, to gain his immense experience of the results of injury and infection of the brain, and of the narrow limits within which they could then be treated, at a time when the sulphonamides, the antibiotics and blood transfusions could not be called in aid and bacterial infection dominated the surgical scene.

The lessons learnt by Jefferson in this war were to make his counsel precious to a younger generation who between 1939 and 1945 were to face the same problems, but this time far better armed with material resources and able to secure striking advances in the surgical treatment of gunshot wounds of the head, which still bear fruit in dealing with the daily toll of head injuries on the roads.

During the second war Jefferson was consultant neurosurgical adviser to the Ministries of Health and of Pensions, and he used his authority and amazing powers of persuasion to secure the establishment throughout the country of neurosurgical centres. It would be difficult to over-estimate the services he thus rendered to neurosurgery, and through this art, to the community in this period. From being a small and subsidiary branch of surgery in this country, neurosurgery is now an independent and well-established branch of practice and research within the wide field of surgery.

In 1926 Jefferson was largely responsible for founding the Society of British Neurological Surgeons, of which he was President and an officer for many years. A medical colleague, at the time of this foundation, named the society the 'Nutcrackers', an informal title it still bears.

At the end of the first world war Jefferson joined the surgical staff of the Salford Royal Hospital, and then of the Manchester Royal Infirmary where in 1926 a small neurosurgical department was provided for him. Yet the opportunities this gave him were scanty, too few hospital beds and inadequate facilities for original study, and for years he had an uphill struggle against narrow working circumstances.

In 1933 he was invited to join the surgical staff of the National Hospital, Queen Square, London, the hospital in which Victor Horsley had in the nineteenth century first practised neurosurgery. It was hoped that this would offer Jefferson all that he needed, and that he would move from Manchester to London. Unfortunately, this proved impossible, and the double role of neurosurgeon to a Manchester and a London hospital, did not allow him to enter fully into the activities of the London institution, and in 1939, being elected to a Chair of Neurosurgery in the University of Manchester, Jefferson retired again upon his original base of work, to the great regret of his London colleagues, who had enjoyed the fugitive glances they had had of him at work over these few years.

Finally, in 1950 a fully equipped neurosurgical unit was opened in the Manchester Royal Infirmary, a year before Jefferson's retirement from that hospital in 1951, and he had the satisfaction of inaugurating it and of leaving it to his successor as a happy contrast to the smaller department in which he had worked for so long.

After 1951, and at the age of 65, Jefferson's life as an operating surgeon had passed its peak, and he entered upon a new sphere of activity, terminated only by his death. He became a member of the Medical Research Council, and the Chairman of its Clinical Research Committee. In both these roles, his great experience of clinical research, of medicine and of surgery, and his shrewd assessment of men and of occasions, made him a most valuable and formative participant in the activities of these important bodies.

His influence in the international world of neurosurgery continued, and by overseas visits, lectures and more formal addresses, and in his innumerable contacts, he was able to aid the advance of neurosurgical thought and practice, and this to a degree achieved by no single man since the heyday of Harvey Cushing's influence in the United States.

He was of an easy temperament, sympathetic with and approachable by young men. He had a gift of persuasion which he knew well how to use, a fine sense of humour and the keenest of wits. His many aphorisms and epigrams became a legend in his lifetime, and will long be cherished. These gems of attic wit and of wisdom do not bear cataloguing in cold blood in a biographical memoir, but they live warmly in the memories of those who heard and enjoyed them. No account of him, however, would be complete that did not mention his contempt of time and timetable, and his fashion of unpunctuality, quite disarming, so unconcerned and gracious was he on ultimate arrival.

Of its nature the profession of medicine is international, and what its members all hold throughout the civilized world is so much greater than what separates them, that an acceptable personality, such as was Jefferson, becomes a most precious ambassador of his country. This indeed he was, and he commanded a deep respect and warm admiration wherever the members of his profession congregated and the barriers that may have separated them as political animals were down.

Jefferson did not lack public recognition of his worth and of his practical achievements. He was honorary Fellow of the Royal Colleges of Surgeons of Ireland, Australia and Edinburgh, of the American College of Surgeons, and of the Royal College of Physicians of London. He was an honorary D.Sc. of the Universities of Cambridge and of Birmingham, LL.D. of Glasgow and of Manchester and Master of Surgery of Dublin. He was elected Fellow of the Royal Society in 1947. Appointed C.B.E. in 1943, he received a knighthood in 1950.

Most of the lectureships for which he was eligible were held by him: The Macewen Memorial Lecture of the University of Glasgow, 1948, the Mallet Purser Lecture of the University of Dublin, 1949, The Balfour Lecture of the University of Toronto, 1950, The Rickman Godlee Lecture, 1952, The Bowman Lecture of the British Ophthalmological Society, 1953, the Lister Oration of the Royal College of Surgeons, 1949, and the Hughlings Jackson Lecture of the Royal Society of Medicine, 1957.

He was an honorary member of the American Neurological Association, the Harvey Cushing Society, the American Neurosurgical Society, and of medical societies in Canada, France, Holland, India, Norway, Russia and Spain.

WORK

It is probably simpler, as readers of these biographical memoirs will appreciate, to list scientific achievement in the more exact sciences than in a biological science. In the former, imposing sequences of equations, marshalled like the troops in a May Day parade in Moscow, follow one another impressively down the pages of the memoir, or chemical formulae coruscate like stars in a printed firmament. Original contributions in clinical science are not thus so easily displayed. Neither equations nor formulae may be a necessary element in them, though this will probably not always be true in the future.

In the period in which he worked, now passing with the increasing applications of laboratory methods to the study of disease, and in the field of his interest, Jefferson had to rely primarily upon the unresting contemplation of the facts of direct clinical observation, and in this order of activity he had a genuine gift of discernment and of interpretation.

Many of his published papers record new facts of observation as illustrative of the principles governing the normal and the abnormal functioning of the nervous system. He was not a mere nosographer, or an exponent of the statistical approach, but in his cast of thought a physiologist.

This is revealed in his post first-war studies on the physiological pathology of gunshot wounds of the head. He analyzed the bacteriology of infected wounds, noting that the *Streptococcus haemolyticus* was the most lethal organism, a finding corroborated by Victor Horsley working at the same time in the Middle East. He traced the spread of infection and the anatomy of its entry into the cerebrospinal fluid pathways. Communication between the lateral ventricle and a focus of suppuration (encephalitis) within the substance of the brain was the common channel of cerebrospinal fluid infection. Thus he established the potent role of ventricular infection in fatal brain wounds, a finding confirmed in the last war.

A later series of studies dealt with the sites of maximal incidence of spinal column injuries, the types of injury found at the different levels, and the different mechanical factors operative at each.

The large intracranial aneurysms on the internal carotid artery in the neighbourhood of the cavernous sinus and the optic chiasma were described fully for the first time in a further sequence of papers. These may be said to have brought these lesions to notice, and thus to have made possible the effective surgical attack upon them.

The diagnosis, localization, behaviour and treatment of brain tumours also owes much to his contributions, and he was an early exponent of frontal lobectomy in the excision of frontal lobe tumours.

His Sherrington lecture at Liverpool on the invasive tumours of the pituitary body embodied much original observation, as did also his various studies on compression of the optic pathways.

In later years he became interested in the disturbances of consciousness associated with brain stem and other injuries. These he discussed with a wide generality of understanding and an appreciation of the psychological and philosophical issues involved in this more than physiological and anatomical problem.

All the physiological inferences that could be drawn from the wide field of his experience as a brain surgeon were lucidly discussed by him in numerous papers over the years. He worked rather by the intensive study of a relatively small material than by the statistical analysis of vast numbers of instances, and thus achieved the deeper level of understanding that this method affords in medicine.

It must also be mentioned that an important factor in his success as a brain surgeon was his profound respect for, and his delicate handling of, nervous tissues. For this gentleness he was renowned amongst his colleagues.

Perhaps, this brief survey of his contributions to clinical science would be incomplete, were it not borne in mind that the search for new scientific knowledge in the operating theatre of a busy hospital is perhaps the most arduous and exacting pursuit that can be imagined. The gruelling physical demands, the over-riding human responsibilities and tensions which dominate operative surgery, are in themselves as much as most men can carry. It is the more remarkable that exceptional men like Victor Horsley in an earlier generation and Geoffrey Jefferson, never a very robust man, in our own, should have retained their persisting eagerness to add to scientific knowledge, and to disseminate it by the written and the spoken word, in addition to the physical and mental burdens of their work, day by day, and year after year.

Only the combination of a high intelligence and immense endurance in a few men makes such a double activity possible. There is nothing cloistered about their lives, and their time is seldom their own. Leisure they know not, or but rarely. They have no 'Long vacations'.

Last years

Although the inexorable passage of the years required Jefferson to relinquish his hospital and professorial activities, he never retired in any real sense. He remained a frequent and welcome visitor to the United States where he continued to influence thought in his field of work, and he never gave up his many contacts or ceased to dispense his wisdom and his wit.

In his last two years he often walked with death, as Sir Harry Platt, his colleague and contemporary, has said, but he continued to live out his life boldly, as though unaware of what he knew must be impending. During this time also, his anxieties were aroused by his wife's failing health, and in the issue these long-joined and devoted partners died within a few days of one another, in January 1961.

In writing this memoir I acknowledge my indebtedness to the writers, his friends and colleagues, of the many appreciations they have given of the man and his work. From them I have borrowed freely.

F. M. R. Walshe

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H. Spencer Jones

HAROLD SPENCER JONES

1890-1960

HAROLD SPENCER JONES was the third child, and eldest son, of Henry Charles Jones, an accountant, and Sarah Ryland, who had been a schoolmistress. He was born in Kensington on 29 March 1890 and exhibited as a child a remarkable interest in mathematics. He was encouraged in his mathematical interests by his parents, and went to Latymer Upper School, from which he won a scholarship to Jesus College, Cambridge. He took a First Class in Part I of the Mathematical Tripos in 1909, and was a Wrangler in Part II of the Tripos in 1911. He then read Part II of the Natural Sciences Tripos, in Physics, and again secured a First. He was second Smith's Prizeman in 1913 and Isaac Newton Student in 1912. In 1913 he was elected to a Fellowship in Jesus College.

In 1913 he was appointed Chief Assistant at the Royal Observatory, which was then at Greenwich, where he remained for ten years, with some interruption occasioned by the 1914-1918 war. During Spencer Jones's first period at Greenwich he went to Russia with C. R. Davidson to observe the eclipse of 1914. He wrote several papers on a subject which remained one of great interest to him, namely the variation of latitude, as observed by the Cookson floating telescope. He also made a determination of the photographic magnitude scale of the North Polar Sequence. In the midst of these activities, he was appointed to the office of H.M. Astronomer at the Cape of Good Hope, taking up office in 1923. He was, of course, a young man to be appointed to such a position, in which he was director of a large observatory.

Spencer Jones threw himself into the business of directing the work of the Royal Observatory at the Cape of Good Hope with great energy. He took office at a time when the staff of the Observatory were in an unhappy state. The Chief Assistant, Dr Halm, was a German; and, during the war, some other members of the staff had quite stupidly and unnecessarily called his loyalty into question. Apart from the ill feeling remaining as a consequence of these disturbances, the staff had other causes for discontent; many of the staff lived in official houses on the site, which had been built in Victorian times and were in need of improvement. Spencer Jones introduced electric light and water-borne sanitation, and secured properly made tarred roads which kept down the dust. As the senior members of the staff, who had been sent out from Greenwich, retired, he replaced them with local men, and was able to secure the services of capable and enthusiastic South Africans. When he left after ten years of office, he had the undivided loyalty and admiration of the entire staff.

Spencer Jones completed a number of programmes which had been started by Gill, and then introduced a number of programmes of his own. Gill had decided that the first use to which the Victoria telescope should be put was a determination of the radial velocities of the brighter southern stars. Accordingly a 4 prism spectrograph was attached to the telescope and many observations were made for nearly twenty years. Spencer Jones set about systematizing and discussing these.

The result was the publication of an important catalogue of radial velocities of southern stars and the calculation of a number of orbits of spectroscopic binaries. He also published a spectroscopic determination of the constant of aberration. Further, in 1925 a nova appeared in the constellation Pictor: a long series of spectra of this nova was secured and described.

However, Spencer Jones's interest in spectroscopic work did not lead him to abandon his interest in positional astronomy, and he removed the spectroscope from the Victoria telescope on Lunt's retirement in 1926 and initiated a programme of photographic determinations of trigonometrical parallax. He took a very active part in the initial observations, measurements and reductions, but he gradually handed over the work to Mr J. W. Jackson. After Spencer Jones's translation to Greenwich in 1933, the parallax work was extended by his successor at the Cape, Dr John Jackson.

Spencer Jones himself observed with the 7 inch heliometer which was Gill's own instrument and which was, by tradition, used by the senior staff of the Cape Observatory. Spencer Jones quickly mastered this instrument and, together with Dr Halm, used it in 1924 to make extensive observations of Mars at its close approach in that year. He used these observations, together with a series of photographic observations made with the astrographic telescope, to derive a value for the solar parallax.

Spencer Jones was now about to undertake his major scientific work, the determination of an improved value of the solar parallax. This was done with the help of the minor planet Eros. In 1901 this minor planet made a close approach to the Earth, at which the minimum distance was 30000000 miles. Observations made on this occasion were reduced by A. R. Hinks, who found a solar parallax of $8\cdot807\pm0\cdot0027''$ from photographic measures and $8\cdot806\pm0\cdot004''$ from micrometer measures. It was realized that a more favourable opposition of the planet Eros would occur in 1930-1931, and in 1928 the International Astronomical Union, which met at Leiden, appointed a Solar Parallax Commission to organize observations of this opposition and appointed Spencer Jones as President of this Commission.

A great deal of work went into the determination of the solar parallax at this opposition. The first step was the determination of the positions of a number of reference stars along the apparent path of Eros. It is not generally realized outside the astronomical profession that the positions of most faint stars have not been determined with the greatest possible precision, and that accordingly if an accurate position is wanted for a special purpose, a special measurement must be made. A system of primary reference stars was observed at thirteen observatories and the observations were discussed with great care by Professor Kopff at the Rechen-Institut in Berlin. These reference stars averaged ten to twelve on an astrographic plate, and were insufficient in number for the reduction of plates on longer focus instruments, and for the purpose of determining the positions of secondary reference stars, four series of overlapping plates were obtained at the Bergedorf, Leipzig, Greenwich and Cape observatories. These were strengthened by incorporating positions secured with a wide-angle camera at the Lick Observatory. A special catalogue of the positions of 5823 secondary reference stars was prepared and published by Spencer Jones and P. J. Melotte.

Observations of Eros itself were secured at 18 northern observatories and 6 southern observatories; but as, at some of these observatories, more than one telescope was used, the total number of telescopes taking part in the Eros campaign was 30. In all 1392 photographic plates were secured at the 18 northern observatories and 1455 at the 6 southern observatories, the grand total being 2847 photographic plates.

The parallax of Eros itself at any time, and therefore of the solar parallax, could be found independently from observations in either right ascension or declination. In right ascension, the solar parallax could be determined from comparison between observations made at easterly and westerly observatories with the same instrument; and in declination, it could be determined from comparisons between observations made practically simultaneously at two observatories, one in the northern and one in the southern hemisphere.

This great mass of material was reduced by Spencer Jones who did not publish the results until 1941. He found for the solar parallax

from observations of right ascension	8·7875±0·0009″
,, ,, ,, declination	8·7907±0·0011″

and gave as his final value of the solar parallax

8·7904±0·0010″.

He also found the value 81·288±0·021 for the reciprocal of the mass of the Moon, 296·776±0·054 for the reciprocal of the oblateness of the Earth and 9·2266±0·0008″ for the constant of nutation. He stated that the last mentioned figure was inconsistent with the adopted value of 9·210″ for the constant of nutation and called for a new, accurate direct determination.

For this monumental work, Spencer Jones was awarded the Gold Medal of the Royal Astronomical Society in 1943, and a Royal Medal of the Royal Society in the same year.

While this work was being reduced, Dyson retired from the position of Astronomer Royal and Spencer Jones was appointed his successor in 1933, being the tenth person to assume the office since the original appointment of Flamsteed in 1675. During his first years at Greenwich he was of course much concerned with the reduction of the Eros material. In addition he took a great interest in latitude variation and in the determination of time. In Dyson's day the former was determined with the Cookson floating telescope, while time was determined with a small reversible transit instrument and kept on Shortt free-pendulum clocks. Spencer Jones determined to substitute for these instruments of astronomical observation an improved version of the photographic zenith tube developed at Washington, and to substitute quartz oscillators for the free-pendulum clocks. But the war broke out; and the photographic zenith tube was not brought into use until after Spencer Jones's retirement in 1955. He had, however, the satisfaction of supervising the operation of the quartz clocks for many years, and of detecting with them variation in the rate of rotation of the Earth. This has important consequences on the apparent motion of bodies in the solar system, which was a topic which Spencer Jones had already studied at the Cape.

In the last part of his tenure of the office of Astronomer Royal, Spencer Jones took a leading part in two matters which were of very great importance to British astronomy; the move of the Royal Observatory from Greenwich to Herstmonceux, and the decision to build the Isaac Newton telescope.

As early as 1938 Spencer Jones had considered the necessity of removing astronomical work from the bad conditions of atmospheric pollution and of interference from city lights that was experienced at Greenwich. During the war, the Time Department was actually moved from Greenwich to the Observatory's magnetic station at Abinger; but even this, as a site for magnetic observations, was too near to electric railways. Spencer Jones pursued the topic of a removal and had the move discussed officially as early as 1944. On account of the impossibility of building accommodation immediately after the war, it was necessary to move, if the move was to be made quickly, to an existing large building. A number of such buildings were examined and the choice fell on Herstmonceux Castle—mainly on account of its superior observing conditions; specifically, sunshine in hours per diem, and number of foggy days per annum.

Herstmonceux Castle was actually purchased by the Admiralty in 1946. During the war it had been occupied by an insurance firm which built a number of huts outside the Castle for office space. These were available for immediate occupation and were in fact used temporarily by the Nautical Almanac Office, but it was agreed that they should be demolished and a new block of buildings erected to house many of the Observatory activities as well as the N.A.O.

In the meantime the Royal Observatory at Greenwich was in a state of suspended activity. The instruments had been dismantled at the outbreak of the war and in fact the Observatory was in the middle of a much bombed district and was itself damaged. After the war, the instruments were not re-erected because of the impending move to Herstmonceux.

Unfortunately the move took many years to accomplish and Spencer Jones retired substantially before the completion of the new building (the

West Block) and consequent demolition of the temporary buildings; and before the removal of the telescopes from Greenwich and their re-erection at Herstmonceux.

He had however the satisfaction of knowing that the telescopes were all fully working three years after he had retired (which was on 31 December 1955) and there is no doubt that his action in pressing for and securing the move from Greenwich to Herstmonceux, and the provision of accommodation, and workshop facilities, on a much better scale than anything available at Greenwich, conferred a new lease of life on the Royal Observatory. The wisdom of the choice of the site at Herstmonceux has been amply confirmed by the observing conditions actually experienced.

Spencer Jones played an important part in the decision to build the Isaac Newton telescope. He was Chairman of the Committee set up by the Royal Society to consider the post-war needs of British astronomy, which recommended the creation of this instrument in 1946; and Spencer Jones himself conducted negotiations with the Treasury, which resulted in the decision to finance the project. Delays of various kinds occurred, however, and Spencer Jones had to retire without the satisfaction of seeing an actual contract placed for the great telescope.

Spencer Jones took a great interest in clocks. When he took up office as Astronomer Royal, the Observatory tested and regulated the Admiralty watches and chronometers, but sent them to outside contractors for repairs. It became increasingly difficult to arrange for satisfactory work, and eventually Spencer Jones set up a repair shop within the Observatory. This now forms an important part of the services which the Observatory performs for the Admiralty. His interest in horology was recognized by the British Horological Institute, of which he became President. He also became a member of the Clockmakers' Company and was twice Master, in 1949 and 1954.

He was the recipient of many honours. He was created a Knight in 1943 and a K.B.E. in 1955. He was highly esteemed in international circles and was President of the International Astronomical Union from 1945 to 1948. He held all the offices in the Royal Astronomical Society, being President from 1937 until 1939 and Foreign Secretary at the time of his death. He was elected an Honorary Fellow of Jesus College, Cambridge, in 1933. (See also Appendix.)

In person, Spencer Jones was tall and dignified, with a certain reserve on first acquaintance that gave way as friendship progressed. He was a ready after-dinner speaker. He was an exceptionally accomplished writer, and possessed the power of writing a formal address, such as a Presidential Address, with great fluency and point in a very short space of time, in such a manner that his first script required almost no correction.

In 1918 he married Gladys Mary Owers. He is survived by her and also by two sons of the marriage. He was elected a Fellow of the Royal Society in 1930.

The author of this notice acknowledges with great pleasure the help he has received from Dr R. H. Stoy, H.M. Astronomer at the Cape of Good Hope, who supplied very detailed notes on Spencer Jones's activities there.

R. v. d. R. Woolley

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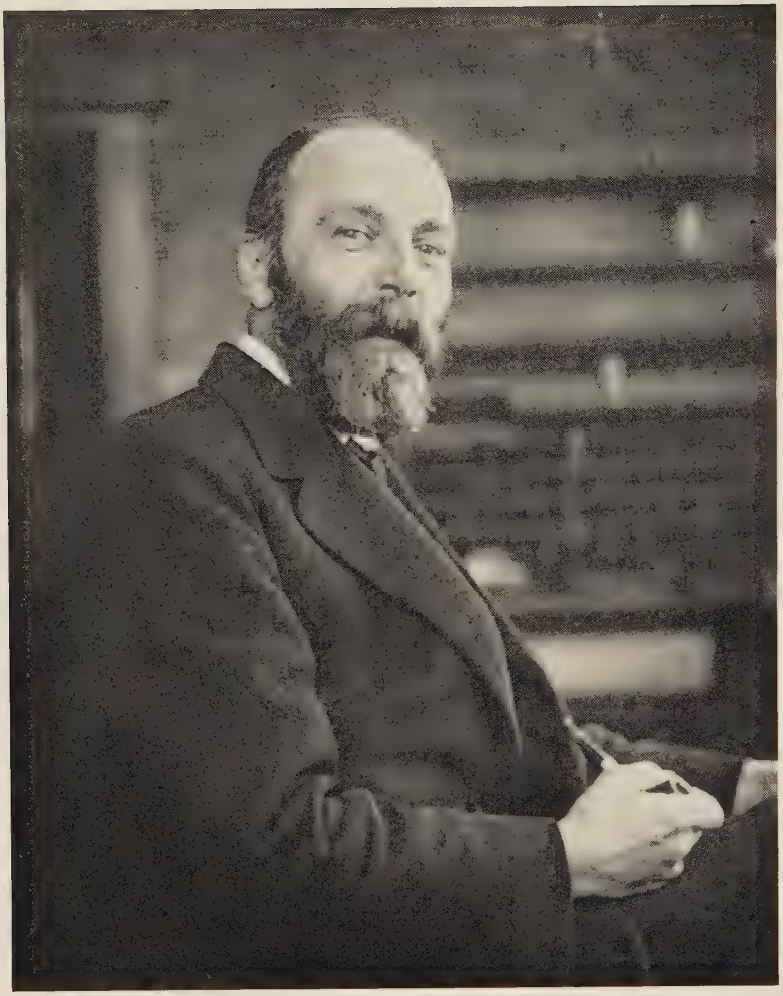
APPENDIX

HONOURS AND AWARDS

Foreign Associate, National Academy of Sciences of the U.S.A.
Foreign Member, American Philosophical Society
Foreign Member, American Academy of Arts and Sciences
Foreign Member, Royal Swedish Academy of Sciences
Foreign Member, Royal Danish Academy
Honorary Member, American Astronomical Society
Hon. Academician, Academy of Coimbra, Portugal
Associate, Académie Royale de Belgique
Foreign Member, Accademia Nazionale dei Lincei, Rome
Foreign Member, Société Royale des Sciences de Liège
Foreign Member, Académie Royale des Sciences, des Lettres et des Beaux-Arts de Belgique
Ehrenmitgliede, Österreichische Akademie der Wissenschaften
Associé étranger, Académie des Sciences, Institut de France
Honorary Fellow, Royal Society of New South Wales
Honorary Member, Astronomical Society of South Africa
Honorary Member, Royal Astronomical Society of New Zealand
Honorary Member, Royal Astronomical Society of Canada
Honorary Fellow, Royal Society of Edinburgh
Honorary Doctor of Science of Oxford, Durham, Brussels, New Delhi Universities
Honorary Doctor of the University of Paris
Honorary Doctor of Philosophy of the University of Copenhagen
Honorary Doctor of Laws, Glasgow University
President, British Astronomical Association, 1933-1935
President, International Astronomical Union, 1945-1948
President, Society for Visiting Scientists, 1946-1952
President, Royal Astronomical Society, 1937-1939
Foundation President, Institute of Navigation, 1947-1949
President, British Horological Institute, 1939-1960
Royal Medal, Royal Society of London, 1943
Gold Medal, Royal Astronomical Society, 1943
Gold Medal, British Horological Institute, 1948
Janssen Medal, Société Astronomique de France, 1949
Bruce Gold Medal, Astronomical Society of the Pacific, 1949
Gold Medal, Stoke-on-Trent Society of Engineers, 1950
Joykissen Mookerjee Gold Medal, Indian Association for Cultivation of Science, 1957
Lorimer Gold Medal, Astronomical Society of Edinburgh, 1953
Rittenhouse Medal, Rittenhouse Astronomical Society, Philadelphia, 1955



William H. Lang

WILLIAM HENRY LANG

1874-1960

THE subject of this memoir did not leave any personal record with the Royal Society, but I have been fortunate in having the assistance of his niece, Mrs Margaret E. Franklin; Professor T. B. L. Webster, who was a close friend for the last 25 years of W. H. Lang's life; the late Lord Stopford, who was also a close friend and was Vice-Chancellor of Manchester University during the last seven years of Lang's tenure of the Chair of Cryptogamic Botany, and Professor Wardlaw, who succeeded to that Chair when Lang retired. To all these I wish to acknowledge my indebtedness for their unstinted help which was the more valuable since Lang was not one who talked about himself and, indeed, was rather uncommunicative about his private life.

W. H. Lang's father was Thomas Bisland Lang, the son of William and Joan Lang who were married in 1825 and lived at Bridge-of-Weir, Renfrewshire. This couple had a typically large nineteenth century family of eleven children of whom Thomas Bisland was the youngest but one. A brother and a sister died in infancy, as was so frequent at that epoch, but Thomas's sister Margaret lived to the age of 89 whilst his sister Mary lived to be 80, which, despite the fact that all but one of William Henry's brothers died before they were 30, indicated that the stock was not potentially deficient in physical stamina. Nevertheless Thomas himself died at the age of 34, only two years after the birth of William Henry. The Baptismal Register records that our subject was born on 12 May 1874 at Withyham, Groombridge, Sussex. It was to this place that William's father had come as a doctor with his young wife to establish a medical practice.

After the untimely death of his father, the young baby and his mother returned to live at Bridge-of-Weir. Thus William was brought up in what must then, nearly 90 years ago, have been quite rural conditions for, even to-day, Bridge-of-Weir has a population of only just over 3000 inhabitants. The nearest city to their home was Glasgow, 14 miles away, a long distance when the only alternative to walking was a horse-drawn conveyance, since bicycles were a rarity till the close of the century.

Lang's mother had originally come from the south of Ireland and William Henry was very conscious of how much he owed to her influence, including the stimulation of his interest in botany and natural history generally. The rural environment of his childhood was also conducive to an interest in biology and at that period almost the only organized instruction available for those with an inclination to study animals and plants was that provided by the medical curriculum.

William's formal education began at the local village school but he subsequently became a day pupil at the Denniston School, Glasgow, where his success is manifest by his becoming Head Boy in 1889. Further, in this Session, when he was only 15, he entered classes at Glasgow University and it would appear that during that same period he also attended classes in the evenings at the Technical College.

It is clear that he must have worked strenuously and with great assiduity for not only did he graduate with distinction, taking his Bachelor's Degree in science in 1894, but the following year he also graduated in medicine and surgery and was awarded the Donaldson Scholarship. On 27 July 1895, when Lang was in his 21st year, he was registered as a medical practitioner but, in fact, never took up practice and thus added one more to the many distinguished botanists whose biological studies rested upon a foundation laid by the medical curriculum.

When Lang went to Glasgow University and joined the botany classes there, he not only came under the stimulating influence of Professor F. O. Bower, whose knowledge of the morphology of the ferns was unrivalled, but he came to a department where, from the time of Sir William Hooker, who was the leading pteridologist of his day, the tradition of cryptogamic botany had been steadily pursued and for Professor Bower this became the major absorption of a lifetime. When Lang entered his classes, Bower had been Professor for only four years; an energetic batchelor of 33 whose recreation, as for one of his predecessors in the Glasgow Chair, Professor Dickson, was classical music. Bower was a 'cellist of no mean ability. In the obituary notice of his teacher, Lang paid tribute to the great debt he owed to Bower's stimulating influence. He wrote, 'I cannot sufficiently emphasize what I owed to Professor Bower from the time I entered his classes' and to this association we can, I think, attribute not merely Lang's own devotion to the study of the cryptogams but perhaps also the interest, if not a real liking, for the classical music which his wife so greatly appreciated.

When Lang took his Bachelor of Science in 1894 he took honours in both botany and zoology and the impression that this student had made on Bower is sufficiently attested by his appointment in that same year as the Junior Assistant in the Botanical Department. The autumn of the year that followed was an important phase in Lang's career for he then went to work at the Jodrell Laboratory of the Royal Botanic Gardens, Kew, under the Honorary Keeper, Dr D. H. Scott, F.R.S., one who Lang characterized as 'a grand Chief'. The Director of Kew at this period was Sir William Thistleton-Dyer and the Keeper of the Herbarium was Daniel Oliver, whose son Francis became one of Lang's lifelong friends. It is perhaps not without significance, in the light of the direction of Lang's studies in later years, that at Glasgow he developed a close friendship with D. T. Gwynne-Vaughan who worked with Robert Kidston on the fossil osmundaceous ferns, whilst here at Kew he forged enduring links with Scott, who was then the acknowleged leader of British palaeobotany, and with F. W. Oliver whose researches on the fructifications of the Pteridosperms were to prove of such importance. It may be added that Lang has paid tribute to the debt he owed to the London botanists of that period, not least among whom may be mentioned Bretland Farmer whose knowledge and enthusiasm for the Hepaticae was a stimulation no one could resist.

At the time when Lang began his career as a professional botanist, plant physiology may be said to have been still in its adolescence. There were as yet far too many facts to be garnered relating to this aspect of botany before they could be woven into a consistent pattern of thought, so that, almost inevitably, many of the more philosophically minded students of botany were attracted by the evolutionary concepts that often served to bring together the wealth of morphological data into plausible, if not always very convincing, integrations. Of such were the two theories concerning the alternation of generations and the relation between gametophyte and sporophyte. One of these regarded haploid and diploid generations as homologous in origin but specialized for the often markedly contrasted conditions of their respective environments. The other held that the two generations were 'antithetic' the sporophyte being, as it were, an interpolation of a diploid phase between successive haploid generations, with an independent evolutionary history, and thus their differences were to be expected rather than explained. Bower, under whom Lang was botanically initiated, was a forceful protagonist of the antithetic viewpoint, whilst Scott, whose influence on Lang came later, was an equally convinced and ardent supporter of the homologous interpretation. Those who advocated the antithetic hypothesis attached great significance, not only to the obvious morphological contrasts between the two generations of most species, but also to the chromosomal distinction between them, a difference the significance of which, however, was greatly diminished by the discovery that aposporous diploid gametophytes could arise directly from the sporophyte and haploid sporophytes from the prothallus. Lang, though never so ardent an advocate of the homologous view as D. H. Scott, realized that the abnormalities which he had observed did much to discount the distinctions between the two generations and the formation of sporangia on aposporous gametophytes which Lang described strengthened his belief that the antithetic view, which Bower held so vigorously and in the atmosphere of which he was brought up, was difficult to reconcile with his own observations. Thus from the time of his experiments with fern prothalli when at Kew and through much of his subsequent research he sought new evidence to clarify the morphological significance of gametophytic and sporophytic organization.

The work on the apospory and apogamy of ferns which he began at the Jodrell was continued when he returned to Glasgow and he drew attention to the striking fact that for all the different species of prothallus which he cultivated, and which under normal conditions produced embryos as a result of fertilization, these sooner or later became apogamous.

In all these earlier papers the theme is informed by the desire to obtain additional evidence. In Lang's concise and balanced statement of the rival theories just referred to, he emphasized the need for further data collected with this objective in mind. The development of sporangia on fern prothalli, the apospory of the liverwort *Anthoceros laevis*, the nature of the prothallial organization in *Ophioglossum* and *Lycopodium*, all of which were investigated by Lang, alike led him to regard the differences between gametophyte and sporophyte as due to evolution in relation to different environmental conditions.

Such an ontogenetic approach was developed by Lang in a paper, published in the *New Phytologist* in 1909, in which he emphasized the diverse environmental circumstances under which the two generations may develop and he instanced the example of *Dictyota* where the two generations have probably evolved under the same environmental conditions and where haploid and diploid organisms are entirely similar in their morphology. The prothallus of *Lycopodium cernuum* with its leafy lobes he regarded as a primitive type, the lobes themselves comparable with the microphyllous foliage of the adult sporophyte. Although he viewed with suspicion any close comparison between pteridophytes and bryophytes, yet the foliaceous character of the free-living gametophyte of the Jungermanniales and the contrast with the non-leafy sporophyte, developing within the enlarging archegonium, were facts manifestly relevant to any consideration of the morphological relationship between the two generations. Lang's ontogenetic theory of alternating generations was, as he emphasized, a particular aspect of a homologous interpretation of the origin of the haploid and diploid phases. No one was more conscious than he, however, of the desirability of an experimental approach to morphological studies and his stimulating address as President of the Botanical Section of the British Association at Manchester in 1915 on the subject of 'Phyletic and causal morphology' made a vivid impression on those who heard it and had it not been delivered during the early throes of the first World War would no doubt have attracted even greater attention than it received.

In this address Lang defined morphology as the scientific explanation of the form, structure, and development of plants; a wider interpretation of the subject than was current at that period, and he deliberately linked the observational data with the physiology, genetics and biology of the organism, whilst he relegated the phylogenetical approach, then paramount in morphological studies of the Pteridophyta, to a subordinate role. He emphasized the problems presented by the sequence of development and the co-ordinated relationships due to the mutual influence of the parts of the whole. Causal explanation as the goal of morphological investigation he endorsed with an expression of purpose that merits quotation both for its own sake and, in our present context, for the light it sheds on Lang's philosophical approach to this field of study. 'A plant is a very mysterious and wonderful thing and our business as botanists is to try and understand it as a whole . . . We have to think of the plant as at once a physico-chemical mechanism and as a living being. To avoid either treating it as something essentially different from non-living matter or forcibly explaining it by the physics and chemistry of to-day.' The former of 'these two crudities', as he aptly termed them, was then a common failing whilst the latter, sometimes in a cryptic form, is perhaps equally rife at the present time.

One of the most interesting of Lang's earlier investigations was his study of the anatomy of the quillwort, *Isoetes lacustris*, the account of which appeared in two papers published in 1915. These displayed in a striking degree his scientific insight and imagination. The genus *Isoetes* comprises some 64 species of pteridophytes, all small, most of which are aquatic plants, but some amphibious, with short stout stems that exhibit secondary thickening and which are clothed above with awl-shaped leaves and pass below into a structure, that may be termed a rhizophore, bearing the dichotomously branched roots in groups. Descriptions of the anatomical structure of *Isoetes* had been given by J. B. Farmer in 1890 and by D. H. Scott & T. G. Hill in 1900, both of which appeared in the *Annals of Botany*. Other significant papers on the subject were published by A. G. Stokey in the *Botanical Gazette* for 1909 and by C. West & H. Takeda in the *Transactions of the Linnean Society* for 1915.

Lang began his paper on *Isoetes* with these words: '*Isoetes* has been studied by many investigators and it might be assumed that its morphology was so well known as to render further description needless.' Nevertheless Lang's morphological acumen led him, in fact, to appreciate features, in particular of the root-stock, which brought into prominence the remarkable affinity of members of this existing genus of ligulate, heterosporous, mostly quite small plants, with the large, similarly ligulate and heterosporous lepidodendroid trees of Carboniferous times. Lang pointed out that the tracheidal organization within the root-stock exhibited a concealed lobing, the number of lobes corresponding with the two, three or four external grooves and that it was not the external and conspicuous lobing which was to be compared with the lobed rhizophore of *Pleuromeia sternbergii*, but the hidden lobing of the tracheidal mass within, the arms of which presented a similarly distinctive grapnel-like form. From the sides of these arms the roots are produced in acropetal succession and through splitting off of the outer cells appear as if superficial in origin. Thus Lang was led to re-examine the stigmarian material in Kidston's collection and found that the roots here also were apparently endogenous in origin, hence further emphasizing the affinity of the recent and fossil groups which Lang regarded as probably more or less directly related—a conclusion that the facts appear to warrant fully. As an outcome of these studies by Lang, the quillworts can be apprehended as 'living fossils' manifesting the remarkable persistence of a striking assemblage of characteristics. It is difficult to escape the conclusion that *Isoetes* trace their ancestry back, according to modern estimates, some 300 million years. An ancient lineage no less remarkable than that presented by the maiden-hair tree (*Ginkgo*) or the Osmundaceae.

One of Lang's colleagues at Glasgow was J. C. Willis, who was Senior Assistant there for two years till, in 1896, he became Director of the Botanical Gardens in Ceylon, whereupon Lang was promoted to the vacated post whilst the Junior Assistantship was filled by the appointment of D. T. Gwynne-Vaughan who remained at Glasgow till his appointment to the Professorship at Birkbeck College, London, in 1907, and during this decade became an intimate friend of Lang, so, together with Bower, they formed a closely linked triumvirate.

In the autumn of 1899 Lang went to Ceylon and Malay to study tropical cryptogamic vegetation and to collect material. The presence there of his former colleague Willis greatly facilitated this project and from this resulted several contributions on the Ophioglossaceae and five on *Helminthostachys zeylanica.* Lang collected and described material of the hitherto unknown prothallus of the latter, and also of *Ophioglossum pendulum* in both of which the gametophyte contains a symbiotic fungus and is wholly saprophytic, thus affording striking contrasts to the prothallus of *Lycopodium cernum*, which Lang later described, with its green leaf-like lobes and which he regarded as a relatively primitive type of gametophytic organization. It is evident that his interest in the diversity of prothallial organization was stimulated by the desire to explore how far this could be related to the different environmental conditions in which they developed and from this stemmed his hope of an experimental approach.

After having served on the staff of the Botanical Department at Glasgow for 15 years Lang resigned in 1909 to occupy the Barker Chair of Cryptogamic Botany at Manchester where the Chair of Botany, in its wider aspects, was occupied by F. E. Weiss who was yet another added to Lang's close circle of palaeobotanical friends.

At Manchester Lang had, for some years, ample time for research, since his teaching duties were concentrated into a single term, although with growing seniority the claims of committees and other administrative duties of the University absorbed an ever-increasing proportion of his time.

A year after coming to Manchester he married his cousin Elsa Valentine, whose parents were Irish and who was very musical and shared with her husband a love for poetry. Lang's wife died, without issue, in 1957, but his niece informs me that to the end of his life he listened to the classical concerts broadcast by the B.B.C. on the Third Programme although it was perhaps the diversities of rhythm, rather than the complex pattern of sound, that constituted for him the major appeal.

Throughout the Glasgow period, from the appearance of Lang's first paper in 1896 till he went to Manchester when he was 41, the subject matter of all his published contributions were the living cryptogams. But we have already noted the close friendships he had formed with those engaged in the study of fossil plants. In 1899 Lang made the acquaintance of the distinguished amateur Robert Kidston, who was an authority of international repute on the plants of the Coal Measures. Some four years later this acquaintance had ripened into a close friendship and in the obituary notice that Lang wrote of Kidston for the Royal Society, he paid him this tribute: that the most important and valued influence in his mature scientific life was the privilege of working with Kidston. For some years previously, Lang's Glasgow friend and former colleague, D. T. Gwynne-Vaughan, had worked with Kidston on the investigation of the fossil Osmundaceae, to which the former brought the professional knowledge that Kidston lacked. It was therefore natural that when this association was brought to an abrupt end, by Gwynne-Vaughan's untimely death, Kidston sought the assistance of Lang in studying the petrified material that had been discovered in Aberdeenshire two years previously. In the September of 1915 Lang went to Stirling to see Kidston and his *Rhynia* slides. Their collaboration was then initiated, but wartime circumstances had brought other claims on Lang's time so that he spent his days at Manchester University on investigations of an official character and devoted his evenings and week-ends to studying the *Rhynia* material. All the time, however, he kept in close touch with Kidston both by correspondence and week-end visits. This fertile partnership persisted till Kidston's death in 1924, after which Lang continued work on the pre-carboniferous fossils, chiefly with material he himself had collected at Thurso.

Kidston and Lang communicated their investigation of *Hornea* in 1918 although the published account did not appear till two years later. This was a rootless and leafless, homosporous cryptogam like *Rhynia* but exhibiting sporangia with a central columella surrounded by a dome-shaped spore mass that inevitably challenged comparison with the similar structure of liverworts. In the same year there were published their investigation of a third and more elaborate type of plant but of the same geological period, namely *Asteroxylon Mackiei* which had microphyllous shoots, suggestive of a lycopod, arising from what Kidston and Lang interpreted as a branched rhizome and which bore pear-shaped sporangia on the ultimate, and apparently leafless, ramifications which were comparable to the less-differentiated aerial plant body of the previously described genera. Without undue emphasis, the morphology of the Rhyniaceae was held to be much more in line with an homologous than an antithetic interpretation of the alternation of generations. These very early fossil plants resembled in their general morphology those described from the lower Devonian rocks of Canada in 1859 by Sir J. W. Dawson but revealed for the first time the structural organization that Kidston and Lang's material presented, with a remarkable degree of completeness. These plants were of great intrinsic interest but it is obvious that for Lang, as for most botanists, their greatest significance was with respect to the illumination they shed upon the general problems of morphological evolution of the plant body.

The horizontally orientated structure that Kidston and Lang described from the material of *Rhynia Gwynne-Vaughani* would appear to have been situated beneath the surface of the soil as a branched creeping organization which they interpreted as a rhizomatous portion of the sporophyte of *Rhynia.*

A recent examination of material by H. Merker has led this investigator to suggest (*Bot. Notiser*, 608-18, 1958, 440-52, 1959) that this structure was in fact the gametophyte of the plant, which interpretation would harmonize with its simple organization, the comparative scarcity of its preservation, upon which Kidston and Lang themselves commented, and the non-septate character of the rhizoids which it bore. It is however important to note that if this structure were a gametophyte it must presumably have been a saprophytic one yet no signs of mycorrhizal hyphae are present within its tissues although fungal hyphae are excellently preserved in the surrounding matrix. Furthermore although the sporophytes of ferns have septate rhizoids, these *Rhynia* plants were not ferns and the most highly organized sporophytes known, those of the flowering plants bear root hairs that are non-septate. This is not the appropriate place to argue the matter in detail but if Merker's postulate resolves some difficulties it raises others equally cogent. The possibility of there being here an ectotrophic mycorrhiza of an extremely primitive type is pure speculation and whatever additional data the future may hold in store, Lang's interpretation would appear entirely justified on the evidence then available.

In this context it may be added that, in accordance with his wishes, Lang's collection of fossil slides is now housed in the Natural History Museum at South Kensington.

From the Australian Silurian rocks, Lang and Cookson described fossils that are still the earliest recorded examples of plants with an undoubted vascular organization. The most highly specialized of these, namely *Baragwanathia longifolia*, possessed a stem up to 6·5 cm in thickness and containing a stellate strand of vascular elements with annular thickenings. This axis bore numerous narrow leaves with axillary sporangia and the general habit must have been similar to that of a modern lycopod. Another genus they described, *Zosterophyllum*, would appear to have had a simple dichotomously branched vegetative organization without any differentiated leaves but this possessed a vascular strand of annular tracheids and the erect branches terminated in racemes of reniform sporangia which dehisced by a transverse split as in a *Lycopodium*. The graptolites present in the same rocks are characteristic Silurian fossils. So that, if we accept the most recent dating, Lang and Cookson's investigations established the existence of vascular plants, exhibiting appreciable complexity of organization, over 400 millions of years ago. These Silurian plants thus give us some idea as to the dawn of the terrestrial flora in the southern hemisphere.

To a remarkable degree Lang combined the scientific approach to detailed investigation with a wide philosophic outlook and represented breadth of culture in its most attractive guise. He was in consequence a versatile and interesting conversationalist who in his own subject could hold the attention of a layman without ever being guilty of talking down to him, whilst the breadth of his sympathies enhanced the interest he evoked. A philosophical approach and a slight whimsicality often endowed his presentation of any subject with a freshness that captured his audience.

In a period when beards, especially long ones, were already uncommon and so beholden with a critical eye, Lang was one of the few whose well-groomed beard, far from suggesting any disguise of effeminacy, did in fact enhance the attractiveness of his classical features and conferred added dignity to his tall figure. He was a confirmed pipe smoker but prone to allow it to go out, especially when talking, so that he was sometimes accused of smoking matches rather than tobacco. Thus it was that the match shortage in the period of the first World War was to him a real hardship and I well recall finding him on St Pancras Station with a long wait for a train to Manchester and no matches. His gloom at the prospect of hours without the solace of tobacco was only equalled by his unfeigned joy when I presented him with a full box, which he told me afterwards he managed to eke out till reaching home.

There is so much that must be recorded of Lang's merits and achievements that to avoid any suggestion of a Victorian epitaph one may mention a characteristic that was a trial to his friends and acquaintances, namely the extreme illegibility of much of his handwriting.

I can recall receiving a post-card from Lang of the contents of which I could only decipher the signature and a date in the middle portion. When I showed this to F. W. Oliver, who knew his writing intimately, he also had to admit his inability to extract its meaning but fortunately recalled that Lang had expressed a hope that I could help him with an examination on the embodied date, a guess which subsequently proved to be correct. Except in this one respect Lang was thoughtful for others, a trait which is, I think, well illustrated by a habit that Professor Walton recalls, namely that when Lang went collecting fossils on Sundays in the Orkneys, in order to avoid giving offence to the inhabitants, he concealed his geological hammer by carrying it with the head held in his hand and the shaft up his sleeve.

Lang could talk interestingly on a great diversity of topics and was always good company whether in the laboratory or in the field. Like his friend Oliver, Lang had a keen appreciation of verbal appropriateness to which his extensive vocabulary administered and it probably appealed to his sense of humour if the *mot juste* was lengthy and recondite.

It was characteristic of the man that he eschewed committees so far as he could reasonably avoid them but, especially after the retirement of Professor F. E. Weiss in 1931 when he became senior member of the Senate, he was persuaded to participate actively in this tiresome but essential part of university activities. Thereafter, his sound judgement and wise council inevitably resulted in an increasing demand for his assistance, which was the more valuable because he had the faculty to foresee any practical pitfalls and the ways in which these could be avoided. When he retired in 1940 the Council Minute, which recorded the University of Manchester's appreciation of his services, referred to the 'innumerable committees of which he had been so helpful a member'. Lang was presented for the Honorary Degree of LL.D. at Manchester on Foundation Day, 20 May 1942, and the words in which Professor T. W. Mason introduced him deserve repetition.

'I present to you one who shows to the world all the outward appearances: the patriarchal presence, the grave dignity, the god-like calm, and the serene wisdom—appropriate to a great scholar—and is all that he appears to be, as this University knows to its profit.' Those who knew Lang best would be the first to endorse this tribute to a personality that was the more impressive because he never took himself too seriously.

It was inevitable that Lang should exercise a general influence upon current thought in botanical morphology but in particular we may note that among those who were students during Lang's tenure of the Manchester Chair two achieved Fellowship of the Royal Society, namely Professor Pearsall and Professor Mather. Professor Irene Manton who came as a Cambridge graduate to Manchester to be a Demonstrator in 1930 was elected F.R.S. 30 years later for her distinguished work on the Pteridophyta. We may here note that she and another Manchester colleague and close friend, Professor Webster, were appointed executors under Lang's will.

Amongst others who have achieved distinction in the botanical field and who were students at Manchester may be mentioned Dr Fletcher, now in charge of the Edinburgh Botanical Garden, Dr Lund, the Algologist at the Fresh-water Biological Station, Windermere, and Dr Purseglove, the Director of Trinidad Botanical Garden.

The Rev J. E. Holloway, the New Zealand botanist who died in 1945 and whose obituary notice Lang wrote, had been an assiduous investigator of the clubmosses, the filmy ferns and especially of the gametophytes and early sporophytic development of *Psilotum* and *Tmesipteris*. It was natural, therefore, that Lang should be particularly interested in these investigations but it was characteristic that he did much to alleviate the scientific isolation of this fellow worker in the antipodes and in no small degree it was due to Lang's influence that the merits of Holloway's painstaking researches became widely appreciated and appropriately recognized.

Beyond his scientific preoccupations Lang had read widely in literature and possessed an extensive knowledge of poetry and philosophy. In his later years he was especially interested in Epicurus. Both he and his wife read French, German, Italian and Swedish, and studied together the poetry of these nationalities. In 1943 they were learning Russian in order to read the Russian poets also.

Lang had a keen artistic appreciation and was himself no mean artist. His holiday sketchbooks contain numerous water-colour landscapes and pen-and-ink sketches with a fresh vigour of style suggesting rapid execution and obviously influenced by the French impressionists he admired. The direction of his artistic leanings is indicated by the Aubrey Beardsleys and Steinlens which adorned the walls of the Langs' drawing room. Lang was greatly attracted also, by the ballet and theatre, the latter interest manifesting itself in the support he gave to the Rusholme Repertory Theatre.

With all his attainments Lang was essentially modest and though he sometimes posed as an egoist was in fact the kindliest of men and endeared himself to those with whom he came in close contact. Lang was elected to the Royal Society in 1911. He was awarded a Royal Medal in 1931 and the Linnean Gold Medal in 1956.

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ERNEST JOHN MASKELL

1895–1958

Ernest John Maskell was born in Cambridge on 1 February 1895. He went to the Cambridge County School (now Cambridgeshire High School for Boys) with a scholarship. His brothers recall that he was very studious, generally preferring a book to games, in which, however, he played his part. His studious inclination, aided by a retentive memory, reaped its reward of prizes. He became a school prefect, in which capacity he showed a quality he retained throughout his life: a marked ability to get on with his juniors, his equals, and his seniors. He had the privilege of being taught by Dr M. Dawson who later became H.M. Inspector of Schools. She encouraged and stimulated his early interest in botany, and Maskell always, and rightly, held her in high regard. When he reached the Sixth Form, or a little later, he, with nine others, formed 'The Honourable Order of the Upper Ten' whose motto was 'Speak truth, live pure, right wrong, follow the Christ the King'. One of their rules was 'That it be incumbent upon every Knight to correspond with every other and with the Rev. C. J. N. Child at least every Christmas'. Many did so maintain contact. Another rule was 'That the Order continue until the death of the last Knight'—Maskell's was the first death. He maintained his connexion with his old school by serving on the Old Boys Society, of which he became President. He was also, for a long time, one of the members of the Governing Body of the school appointed by Cambridge University.

At the age of eighteen he went to Emmanuel College as an Entrance Scholar. In 1915 he was placed in the First Class of Part I of the Natural Sciences Tripos and was awarded the Frank Smart Prize in Botany. At this stage his academic studies were interrupted by his joining the Friends' Ambulance Unit. In this service he formed a close friendship with a Fellow of his college, Mr L. H. G. Greenwood, who writes: 'I first came to know him when he was an undergraduate here before 1914, but I remember little about that time. We became close friends during the war at the military hospital in York, mainly staffed by the Friends' Ambulance Unit, of which we were both members. In the often trying conditions of service there, his unselfish and co-operative friendliness, together with his complete efficiency, did much to make things go smoothly and happily. Later, for a shorter time, we were together at the hospital in Courtrai also run by the Unit, and the same remarks apply to his service there.'

When the war was over he returned to Cambridge to resume his academic studies. In 1920 he was placed in the First Class in Part II of the Natural Sciences Tripos (Botany) and was awarded the University Frank Smart

Studentship. He was one of those students who had the fortunate experience of listening to the lectures on plant physiology given by F. F. Blackman. It would have been strange if Maskell had not responded. He became a research student under Blackman's supervision. With wise guidance and his own ability Maskell laid a secure foundation for a successful career of research and later of teaching. Before he had finished his work for the Ph.D. degree he joined the Horticulture Unit, housed in the School of Agriculture at Cambridge, as a plant physiologist, but before he had really started a research problem he moved to the Rothamsted Experimental Station. Here he had the advantage of contact with R. A. Fisher, from whom he rapidly acquired a sound basis of statistical knowledge at a time when few biologists appeared to realize its importance. He later made good use of this acquisition. After a short stay at Rothamsted he joined T. G. Mason in 1926 at the Cotton Research Station newly established in Trinidad. Judged by the publication of important papers on translocation this was a most fruitful collaboration. Unfortunately Mason died soon after Maskell and had no opportunity to give in writing his personal impression of Maskell's contribution. This short spell produced most of Maskell's work that has been published. Dr E. Phillis, who succeeded to Maskell's post when he left for Cambridge in 1931, and who overlapped with Maskell for about three months and saw him again in Trinidad when Maskell spent a sabbatical leave there, writes: 'Maskell's contribution was the introduction of precision into the more general views of Mason. Maskell was responsible for the statistical side of the work, plus the methods of routine analysis. He was also responsible, I believe, for the training of the local staff, who formed an extremely competent group. He gave them an understanding of the problem they were working on, and was remembered with great affection by many people of all grades in St. Augustine and district for many years after he left'.

Meanwhile Blackman was anxious to build up a nucleus of plant physiologists in Cambridge and he secured Maskell's return to the Botany School, where he became a Lecturer in 1931. Here he spent the next twenty years of his life. If it was not productive of published papers it was in many other ways. At first he gave special post-graduate lectures on a variety of topics in plant physiology. Later he took over Blackman's elementary lecture course on the physiology of the cell, and when Blackman retired from his Readership in Botany in 1936 he became responsible for the biochemical portion of Blackman's lectures in the advanced course. For a time he conducted an elementary course in practical plant physiology. Throughout he supervised the training in research of a succession of research students. He played a full part in helping in the successful running of a scientific department, not the least of which was the Physiology Club for research students, now called the Blackman Club. This, and his lectures, involved wide reading, and those who had the privilege of being members of the Club and of attending his lectures will remember to what good use a wide reading, filtered through an able mind, was put. At his lectures the blackboards were filled before the lecture with a mass of factual detail, neatly arranged in tabular and graphical form. The result was that the keener students, who could find the time, would come before the lecture to copy the data. He rightly valued his teaching experience, not only for the help which he was able to give to others but for the ideas it gave him for further research work.

His activities were not limited to his service to his department; he led a full life inside and outside the University. His ability and kindliness fitted him well for this. During the Second World War he found time to serve his college as acting steward and the college gave him further opportunities by electing him to a Fellowship. He served on that hard-worked body the General Board of the University on which he played a full part, serving on many of its special committees.

His distinction as a plant physiologist was recognized by his being made a Reader and later when the Headship of the Sub-Department of Plant Physiology became vacant he was appointed to this. He was elected a Fellow of the Royal Society in 1939.

His happy and fruitful spell at Cambridge came to an end in 1951 when he succeeded to the Mason Professorship of Botany at Birmingham University on the retirement of Professor W. Stiles. Unfortunately his early death in 1959 resulted in his tenure being short. Nevertheless his unceasing activity enabled him to contribute much. He co-ordinated the efforts of his colleagues in the planning of the new Biology Building and displayed his wide botanical interests and love of plants in the management of the University Botanical Garden.

His parents were Baptists but he joined the Society of Friends when an undergraduate. Despite all his other activities he devoted much time to service to the Society. For many years he was a much-appreciated Elder of the group at Cambridge whose Meeting House is in Jesus Lane. Friends record that his 'vocal ministry', which was most helpful and simple, has left vivid memories. In it he made great use of quotations from poetry—poetry and pictures were a great source of inspiration for him. He served on the Governing body of the Friends School at Saffron Walden and Mr Howard Diamond, who was the Chairman of the Governors, writes: 'Maskell served as a Governor of the School for eighteen years from 1933 to 1951. His work on the Governing Body was appreciated because he formed a link between the School and the University, and was able to advise senior boys and girls regarding their preparations for university entrance. Needless to say, the staff also turned to him, not only for help and advice on his own subject of botany, but to draw on his wisdom and knowledge on all educational matters. Maskell's interest in the life and work of the school stemmed from the fact that it set out to help all its scholars, whether members of the Society of Friends (whence the foundation arose) or Essex County scholars, to secure the best available grammar school education without reference to the means of their parents; and to this end he gave generously. No doubt reference will be made elsewhere in the memoir to Maskell's deep appreciation of poetry, especially modern poetry. This gift he sometimes extended to the school community at Saffron Walden, where he would talk to the school, and especially to the older scholars, in a way which fired their imaginations and lifted their thinking into a new range.'

He had close contact with the late Professor Sir Arthur S. Eddington, F.R.S., and was active in the establishment of the Arthur Stanley Eddington Memorial Lecture.

Outside the University and the Society of Friends he gave generously of his time and wide experience in service on committees—Agricultural Research Council (1952-1957), the executive committee of East Malling Research Station, the governing body of the Grassland Research Station, the Scientific Advisory Committee of the Empire Cotton Growing Corporation, the Advisory Panel of the Indian Tea Research Association, the governing bodies of Studley Horticultural College and King Edward's School, Birmingham. To these he brought not only knowledge but wisdom. It is not surprising that with all these activities he sometimes seemed a tired man, but he derived refreshment from his home life and his religion. The end of a holiday always saw him full of vigour again.

In 1922 he married Elisabeth Rose McCormick, who shared many of his interests outside science, particularly his love of gardening. They made their home and garden a place where he and their many friends could find refreshment.

Their only son unfortunately suffered from physical handicap. The anxiety this caused and the sad blow of his death soon after his entry to the Leys School were borne with great fortitude.

They were hoping to enjoy his retirement by returning to Cambridge, but his tenure of the Chair at Birmingham was dogged by illness. He was seriously ill with pneumonia in the spring of 1958 and, though he returned with characteristic determination to carry on, he probably went back too soon. He died a few months later on 20 December 1958.

Translocation in plants

Maskell's main published contribution to plant physiology is that on translocation in plants. When Maskell joined T. G. Mason at the Cotton Research Station, Trinidad, they began the series of researches on translocation of carbohydrates, nitrogen and minerals in cotton for which they are chiefly remembered. Mason, while in Dublin, had already worked on the problem of translocation, confirming in yams the very rapid transfer of dry weight that Dixon had found in potatoes, and which had led Dixon to question the long-accepted belief that organic nutrients were transported in the bark. The measured transfers of dry weight, 3 to 5 grams per square centimetre of phloem per hour, implied a flow of solution of, say, 10 per cent sugar at velocities around 100 cm/h through the sieve-tubes and fine sieve pores, a possibility that Dixon denied. He proposed instead that tissues other than the bark, probably the wood (xylem), must be contributing largely to the transport, thus beginning a controversy that was finally resolved by Maskell and Mason in favour of the phloem.

Mason probably initiated the study and it is not difficult to read between the lines of the opening sentences of their first paper a decision to devote the practical resources at their disposal to this important theoretical end: 'Such problems as the shedding of flower-buds and young bolls, the condensation of sugar to form cellulose in the lint hair, and the variations in habit and development as a result of climatic and other causes could not be profitably attacked until a great deal more information was available relating to the machinery responsible for the transport of sugars and the factors determining the rate and direction of movement within the plant-body.' And their plan of campaign is clearly stated: 'An adequate theory of translocation involves a solution of at least four problems: (1) The channel of transport; (2) the factors determining the direction of movement within that channel; (3) those determining the rate of movement within the channel; (4) those determining interchange between the channel and other tissues.' They solved the first of their four problems at the broad histological level; the remaining three are still unsolved after 34 years.

Earlier ringing experiments, in which either wood or bark was removed and the effect on the growth of plant parts beyond the ring determined, had not unequivocally decided which was the channel of transport. Although removal of a ring of bark caused growth to cease, adherents to the wood-theory objected that the ringing might also have damaged the wood, particularly the outer region. Mason and Maskell measured the variations in amounts of total carbohydrate and of sugar in the leaves, bark and wood, expecting that the transporting tissue would exhibit fluctuations in sugar content closely following those in the leaves. They found that it was the bark not the wood that showed daily fluctuations in sugar content parallel with, and closely following, those in the leaves. This still left the possibility that sugar might move in the wood and be transferred into the bark, but this was eliminated by experiments showing that the transport still went on into flaps of bark separated from the wood, but attached at their upper end.

To this careful, clear and decisive work, Maskell's contribution most probably included the comprehensive statistical design and the application of Student's *t* test to establish the significance of the differences measured. The results laid the foundation of all subsequent work on translocation until the advent of radio-isotopes made possible approaches of a different kind.

Their next series of experiments was directed towards the solution of the second and third of their stated problems; the factors determining the rate and direction of movement of sugars. From their earlier work they had drawn an analogy between the translocation process and simple diffusion in that the sugars moved down concentration gradients, but enormously faster than by diffusion. They set out to determine just what this acceleration amounted to, and how far the analogy could be pressed. They found that movement of sugar was always down a concentration gradient of sugar, and that reverse movement in the stem and reversal of the concentration gradient could be induced by changing the positions of source and sink. They could not, however, demonstrate movement of sugars into darkened mature leaves. Their analyses and correlations showed further that the mobile substance was sucrose and that it was probably moving in the sieve-tubes. The rate of the movement was found to be $2-4\times10^4$ times what would be expected from diffusion in aqueous solution.

The mechanism by which this high rate is brought about was and remains obscure. In the polemics of the period there were four schools of thought: that of Münch believing in the mass flow of sugar solution under pressure through the sieve-tubes; that of Curtis believing in the circulation of the protoplasm in the sieve-tubes; that of van den Honert believing in the spreading of molecules by surface tension over interfaces; and that position to which Maskell and Mason found themselves driven by their adherence to the diffusion analogy, and which received the name 'activated diffusion' hypothesis. This was the most vague of the four since no tangible mechanism was envisaged and no testing experiments could be done. Consequently it has not suffered the discredit that has overtaken the other three.

Maskell and Mason then turned their attention to the transport of nitrogenous substances in the cotton plant. The very many samples already collected for the carbohydrate work were still available, and by analyzing these for nitrogen they economically built up a picture of the daily nitrogen balance of leaves, bark and wood. The analyses suggested that organic nitrogenous compounds were formed in the leaves from nitrates imported via the wood, and the organic nitrogen exported from the leaves through the bark. The rate of movement (calculated as grams of asparagine per square centimetre of phloem per hour) was about one-tenth of that of the carbohydrates.

Having established these similarities of nitrogen movement to carbohydrate movement, they set out to investigate the relation of movement to concentration gradients, and the 'acceleration of diffusion'. The problem was a formidable one: 'It will be seen that the solution of this problem involves the solution of a number of subsidiary problems, viz. (1) the channel of transport, whether the sieve-tubes or some other tissue, (2) the nature of the mobile compound or group of compounds, (3) some estimate of the concentration of the mobile form in the channel of transport. With the technique available hardly one of these questions could be settled independently of the others. The problem resembles that of the solution of a set of simultaneous equations.' From a mass of analyses of six nitrogen fractions vertically in leaf, bark and wood, and transversely through the bark, they could find no support for their gradient hypothesis. Any gradient they could detect was negative in the direction of transport, so they were led to postulate a positive gradient of mobile compound(s) in the sieve-tubes masked by negative gradients of storage nitrogen in other tissues of the bark. They further investigated the changes in gradient accompanying changes in movement: 'If we stop transport or reverse its direction we should affect only, or mainly, the dynamic gradient of translocatory material. While, therefore, the net gradient may be negative, the *change* in the net gradient, when the rate or direction of transport is altered, should be positively correlated with that change in movement, and should be a measure of the change in the dynamic gradient.' Elaborate analyses and reasoning produced some support for the idea of a masked positive gradient of mobile nitrogen, for reversing the direction of transport resulted in a steepening of the original negative gradient. This, they argued, was due to the reversal of the masked positive gradient which was now added to the masking negative gradient, not subtracted from it. In so far as quantitative estimates were possible the acceleration of nitrogen diffusion appeared similar to that of carbohydrates. 'This suggests that the acceleration mechanism in the sieve-tubes acts impartially on sugars and nitrogen compounds.' Of the nitrogen fractions analyzed, they could not identify any one as wholly static or wholly mobile. Further attempts to identify the mobile compound(s) by observing changes in nitrogen fractions where the bark was interrupted by a ring led to a similar conclusion, viz. that all nitrogen fractions including the labile protein contribute to the longitudinal movement in the sieve-tubes. This conclusion was perhaps inevitable because of the fact that the different fractions proved very labile, easily converted from one form to another within the bark more rapidly than they moved.

Maskell and Mason next turned their attention to transport of mineral nutrients. Having shown that nitrogen from the roots was transported to the leaves by the wood as nitrate, elaborated, and exported as organic nitrogen to other parts of the plant, they were anxious to find whether phosphorus and metallic ions followed similar paths. By the well-tried techniques of analyses above and below rings and reversals of source and sink, they showed that this pattern of distribution held for phosphorus and potassium, and that *en route*, there were extensive exchanges of the transported nutrients between the mobile and various immobile phases. Redistribution of calcium from the leaves was very slow. The gradients of mineral concentration were not always the same as the direction of movement, and the masking-static-gradients were again invoked.

By now the controversy between the several protagonists of the mechanisms already mentioned was reaching a head. Maskell and Mason had so far remained uncommitted to any definite mechanism for the acceleration of diffusion, but in the literature of the day much space is devoted to 'the critical experiment' which would decide between the mass-flow hypothesis and its opponents. One such was to demonstrate transport of two substances in opposite directions simultaneously; and Maskell's last contribution to the joint work was his part in an experiment of this kind. They failed again to make mature leaves import carbohydrate, so could not show that nitrogen was coming out while carbohydrate was going in; but in the stem they were able to demonstrate upward movement of stored organic nitrogen at the same time as carbohydrate was moving in the opposite direction. The argument was not settled, however; the adherents of the mass-flow theory shifted their ground and suggested that different sieve-tubes were specialized for transport in different directions.

Not the least important contribution to plant physiology were the ideas he developed in his lectures; ideas partly derived from, and forming the basis for, much of the research done by students under his supervision. Unfortunately his development of these ideas is unpublished.

Carbohydrate metabolism

Carbohydrate metabolism had long been a field of special interest to plant physiologists in the Botany School at Cambridge, and it was thus natural that Maskell should begin in 1933 a general investigation of the metabolism of carbohydrates in plant tissues. His first student, A. G. H. el Gawadi used carrot slices held either in water or in sugar solutions and this work led to the recognition that the sucrose of the medium was inverted either in the external solution or in some readily accessible part of the cell. A careful analysis of the changes in the amounts of sucrose and of the hexose sugars in the tissue led Maskell to recognize a balance between these sugars. Earlier workers had noted that the proportion of sucrose to the hexose sugars might vary widely either in the same plant tissue at different times or as between different plant tissues. It was, however, Maskell who first recognized the form of the balanced relation commonly shown between sucrose and the hexose sugars in plant tissues. This balance has the graphical form of a Michaelis rectangular hyperbola when hexose content is plotted against sucrose; in low concentrations of hexose, sucrose may increase nearly proportionally with increase of hexose, in higher concentrations of hexose, however, sucrose increases greatly for a small increase of hexose. Maskell observed this form of relation in carrot slices, both when the slices were held in water under starvation conditions and also when the slices were fed with either sucrose, glucose, or fructose in the surrounding medium; the balance was also maintained under anaerobic conditions. A similar form of curve was shown in leaves attached to the plant during a 24-hour period during which the sugars first increased because of photosynthesis and later decreased because of translocation.

From all the data obtained with carrot, Maskell obtained a curve of best fit, which was closely similar in form to that of a Michaelis rectangular hyperbola, the curve being nearly parallel to the sucrose axis in high sugar content. Maskell recognized that this balance represented a special case of the dynamic state common to all chemical reactions occurring in living matter. The following formulation, though set out for the sucrose/hexose system, may thus be of wide general application. Maskell formulated this observed relation as being due to two opposed series of reactions, as shown below:

$$\text{sucrose} \underset{\text{condensation}}{\overset{\text{hydrolysis}}{\rightleftharpoons}} \text{glucose}+\text{fructose}$$

He then treated each series of reactions as being enzymic; taking Hy as the concentration of the products of hydrolysis and C as the concentration of the condensed product sucrose, V_c and V_{hy} as the maximum rates of condensation and hydrolysis, K_c and K_{hy} as the Michaelis constants for the condensing and hydrolyzing reactions, he obtained the following equations:

$$C = V_c K_c \frac{Hy}{V_{hy}K_{hy}+(V_{hy}-V_c)\ Hy}$$

$$\text{or } (C/K_c) = \frac{(V_c/V_{hy})\ (Hy/K_{hy})}{1+(1-V_c/V_{hy})\ (Hy/K_{hy})}.$$

Thus if V_c is greater than V_{hy}, C increases to infinity as the concentration of the hydrolytic product approaches asymptotically a maximum value

$$Hy_{\text{max.}} = \frac{K_{hy}}{(V_c/V_{hy}-1)}.$$

At that time the biochemistry of the synthesis of sucrose was not understood, but Maskell realized that energy would be needed for the synthesis and he therefore developed a further formulation in which the rate of synthesis was linked with the rate of respiration. Moreover, since the rate of respiration is often correlated with the concentration of sugar, a second formulation was given where the rate of synthesis is linked with the rate of respiration, the latter being correlated with the concentration of sugar.

Maskell's other great interest was in the control of the rate of respiration. At a surprisingly early date he realized that the Pasteur effect could be due to the controlling influence of (a) the concentration of inorganic phosphate and (b) the adenosine triphosphate/adenosine diphosphate ratio on the rates of glycolysis and of the tricarboxylic acid cycle. He therefore suggested that K. S. Rowan should determine the changes of *ATP* content when green peas were transferred from air to nitrogen. Peas were a good choice of material since they contain a large amount of *ATP* and Rowan was able to show that the *ATP* content decreased in nitrogen. The Pasteur effect in peas was thus associated with a decrease of *ATP* in nitrogen and this compound may well retard glycolysis in air by a back-reaction. Another controlling factor must, however, be the concentration of fructose-1,6-diphosphate and the content of this compound was shown to increase in air. While, then, the complete elucidation of this complex mechanism must await further study, Maskell undoubtedly made a considerable contribution to its understanding.

A special aspect of the control of respiration rate in plants is the so-called climacteric stage in various ripening fruits where the onset of ripening is associated with a large increase in the rate of respiration. Maskell suggested that the phosphate mechanism might also control the onset of the climacteric and this idea stimulated the work on this subject since carried out both by C.S.I.R.O. at Sydney and by Biale's group in California.

Maskell's lectures to second year and Part II students were based little on text books, but depended largely on his own work. His great interest was in the dynamic state of metabolism and this subject was clearly discussed in his lectures.

Information about Maskell's boyhood was kindly supplied by his brothers, Mr H. Maskell and Mr A. Maskell, that about 'The Honourable Order of the Upper Ten' by Mr L. V. Slater, one of the original members, and that about his membership of the Society of Friends by Mr A. K. Dewey.

The account of Maskell and Mason's work on translocation is by Dr M. J. P. Canny, and that on carbohydrate metabolism by Dr J. Barker.

G. E. Briggs
M. J. P. Canny
J. Barker

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C. E. K. Mees

CHARLES EDWARD KENNETH MEES

1882-1960

Photography was about sixty years old at the end of the last century. Although it was intended to be used for recording portraits and scenes, the possibility of using it as a recording medium for science was also realized from the start, and through the years it became more and more relied upon as a way of making records for observation and measurement. It was not until towards the end of the century, however, that studies were started on what is now known as 'the theory of the photographic process,' which embraces the relationship between the photograph and the thing photographed. Until then the scientific world had shown little interest in the science of photography as distinguished from its practice.

The science of photography deals with the physics and chemistry of light-sensitive substances, especially of silver compounds. It is concerned with the nature of the light-sensitive material, the changes occurring when it is exposed to light, the chemical actions involved when a developer is applied to bring out the image, the structure of the developed image, the faithfulness of reproduction of the brightness scale of the original subject, and so on.

The first worth-while study of the sensitivity of photographic materials, and the formulation of the rules by which the developed image is related to the original subject, were published by Ferdinand Hurter and V. C. Driffield in 1890. Their investigations, described in the *Journal of the Society of Chemical Industry*, rested in oblivion for some time. At the end of the century, however, two young students became interested in the subject, eventually found the Hurter and Driffield paper, realized its significance, and very substantially broadened its subject matter.

These students were Charles Edward Kenneth Mees and Samuel Edward Sheppard. Both were at St Dunstan's at Catford and both went to University College, London, where they passed their B.Sc. and D.Sc. degrees for work on the theory of the photographic process. They then separated, Sheppard having been awarded one of the 1851 Exhibition Scholarships, on the strength of which he went to Marburg with Karl Schaum and to The Sorbonne with Victor Henri. Mees continued in photographic research and the application of photography to scientific and technical investigation, and also became a pioneer in the application of science to the problems of industry. Sheppard joined Mees again later to investigate for the rest of his life many of the fundamental aspects of the photographic process.

Charles Edward Kenneth Mees was born on 26 May 1882, at Wellingborough, Northamptonshire. He was the eldest child, and had two brothers, and a sister who died in infancy. His father was a Wesleyan minister with a Bachelor of Arts degree from London University. His mother, Ellen Jordan, had a number of Wesleyan ministers among her ancestors.

Since a Wesleyan minister retained a church for only three years, his family did not acquire a rooted attachment to one locality, and Mees never returned to Wellingborough after he left it when he was one year old. The family moved to Ipswich, then to Colchester, and then to Hythe when Mees was seven. Here he started school. He had already been taught the elementary subjects by his family, and was particularly fond of reading history, biblical for the most part, and he stated that he had no difficulties therefore in coping with the work of the small schools.

'A child in a Wesleyan minister's household receives a very thorough training in the literature and history of the Bible, which produces a permanent impress on literary style. At the same time, circumstances are such that the child learns early to speak in public, as a result of which I have never known any form of stage fright and have always been prepared to say what I had to say without hesitation.'

When Mees was ten, at Barnet Grammar School, it was discovered that he was very shortsighted, a fact which explained his difficulty in following class work. He was, however, introduced to science in the form of elementary chemistry, read books on it, fixed up a small laboratory at home, and had no difficulty in leading his class in chemistry. He was a weakly child, not interested in boys' school games and outdoor life, and turned to books and the French language. In 1894 he went to Kingswood School in Bath, a school which had been established by John Wesley for the sons of Wesleyan ministers. 'The school was excellent from an educational standpoint but proved very bad for me physically. Because of the poor food, I suffered from dyspeptic indigestion, which has been a nuisance ever since.'

Since Mees did not mix too well at that time with the other students, he found himself left to his own devices and gravitated to the laboratory. In the three and a half years that he was at Kingswood he acquired a good knowledge of elementary chemistry, especially qualitative analysis, and he expanded his laboratory at home. The headmaster at Kingswood thought there was a danger of Mees becoming too specialized in chemistry, although this had no effect on Mees's views. However, the classical master introduced him to the study of the variant readings of the New Testament in Greek. This was the only classical subject in which he became interested and of which he retained any memory. Later in life he was greatly interested in archaeology but he could not relate it to his classical studies at school.

Attacks of illness continued, diagnosed as 'rheumatism owing to climate,' and so in 1897 he was transferred to Harrogate College, a private boarding school. He felt that the teaching was inferior, although the climate, accommodation, and food were better, but the training he had had at Kingswood made it easy for him to pass the London matriculation examination in June, 1898.

Mees's family then moved to Croydon where his father was Chaplain of the Guards' Depot at Caterham. This made it convenient for him to attend as a day student St Dunstan's College, Catford, a technical school. C. M. Stuart, the headmaster of St Dunstan's, was a chemist and a pupil of Fittig, and with Mees's father he decided that Mees should go to University College, London, and do chemistry under Sir William Ramsay. In 1899 Mees entered his name for the Andrews Scholarship examination at University College, as well as for the Intermediate B.Sc. of London. He passed the Intermediate, second class, but failed to get the scholarship. Ramsay said he was too young in any case and must wait another year before entering his classes.

Mees now had a good laboratory at home and, at Stuart's suggestion, started some research on a project in which he was not interested, an organic synthesis, but at least it helped him to resolve problems in his own way. In 1900, Mees obtained the Andrews Scholarship, together with an extra one from Ramsay, who looked through his notebooks, said he had done all the routine practical chemistry required for the Bachelor's degree, and that he could start to teach him to do research.

He was assigned as an assistant to E. C. C. Baly, who was photographing the spectrum of the iron arc with a 10-foot Rowland grating in connexion with the study of the spectra of the rare gases. This was his introduction to scientific photography, although a great deal of his time the first year was taken up with classes in chemistry and physics. H. M. Callendar was the Professor of Physics and his lectures had a great influence on Mees, particularly in connexion with the detection and evaluation of errors. He did a good deal of physics laboratory work under A. W. Porter and H. Eumorfopoulos, since physics was his subsidiary subject. Mees's outstanding contact at University College, of course, was with Sir William Ramsay whom he regarded as an inspired and inspiring teacher. Lord Rayleigh and Ramsay had discovered argon and Ramsay and Travers had identified the noble gases of the atmosphere—helium, neon, argon, krypton, and xenon. The environment was steeped in the noble gases. Norman Collie was the Professor of Organic Chemistry and predicted that the bright red glow produced by an electrical discharge through neon would one day be used for street lighting. The new work on radium emanation was starting and the students were proud to be allowed to help Ramsay in operating the Toepler pumps.

Mees's contact with the scientific group at University College in the early years of the century produced a profound effect on his attitude to science and scientific research later on. Even though the equipment at University College was meagre and inferior, the spirit of research and study was very infectious, and it was a good place for a student anxious to spend most of his time on laboratory work.

In the summer of 1901 Mees had an illness involving an intermittent high fever which left him very weak after each attack, and it seemed doubtful if he would be able to return to college. 'The doctor said it was due to overwork, which was certainly incorrect.'

Ramsay felt that Mees was probably working too hard for the Honours B.Sc. degree. At the same time Ramsay was a strong agitator at the Senate of the University for students being granted degrees for their research, and the first students approved by the Board of Studies of London University to enter for the B.Sc. degree by research were Sheppard and Mees. They had been interested in the theory of photography and Ramsay suggested it would be a suitable subject for a research thesis. Practically nothing in the books on photography made use of the physical chemistry of the time, and Mees's professors and fellow students also knew practically nothing of the subject. Sheppard, who was a very good searcher of scientific literature, turned up the Hurter and Driffield paper of 1890 which was entitled 'Photochemical investigations and a new method of determination of the sensitiveness of photographic plates,' and this came as a revelation to the two students, for here were people who had really studied photography scientifically. They had found a way of measuring the image produced by exposure to light followed by development, and of measuring the rate of development of the exposed image. Here was a model for the attack on the nature of the photographic process.

The Hurter and Driffield apparatus was unique but rather primitive, and the advances in physical chemistry since 1890 enabled Sheppard and Mees to improve on their experimental methods. Mees wrote in his thesis in 1903, 'In view of this divergence in the methods and theories at present in use . . . I deemed it necessary to largely repeat the work of Hurter and Driffield, considerably amplifying it, and, wherever possible, making it more accurate.' Sheppard and Mees did this and both got their Bachelor of Science degrees by research in 1903. Their theses included repeating and extending the investigations of Hurter and Driffield, and in particular studying the general kinetics of development and especially the determination of the velocity of development.

They had built a sector-wheel exposure machine, a developing thermostat from a bucket, and a standard light source in the form of an acetylene burner, which gave Mees 'a great respect for acetylene.' This burner remained in use as a practical standard in sensitometry until the early thirties. To measure densities they used a Hüfner spectrophotometer borrowed from E. H. Starling, Jodrell Professor of Physiology. (It is understood that he had borrowed it from Guy's Hospital, so its origin is not clear. Mees later redesigned the spectrophotometer with Frank Twyman and used the new instrument in later work. It was made by Adam Hilger.)

'It was really only an orientation of the field, but the work we had done in two years was sufficient to show us that much more work could still be done, and we felt that by then we knew how to do it. Again, we both suffered and gained by the fact that nobody around us knew anything about the subject, and that we had to work out all of it. The general opinion at University College was probably that we were extremely eccentric and rather undesirable students.' It is understood that Eumorfopoulos used to refer to them as 'the heavenly twins'.

Mees's first scientific publication was in 1902 in *Chemical News* and was actually nonphotographic. It was, however, from the Chemistry Laboratory of University College and dealt with a modification of Schrötter's CO_2 apparatus.

Mees was anxious to start a career and earn a living, but his father felt he should continue research work and take the D.Sc. before leaving college. It seems probable that Ramsay supported this. It was decided that Sheppard and Mees would continue their collaborative research with entirely new and efficient apparatus.

There was not adequate space at University College for photographic work on the desired scale. Mees, therefore, built a laboratory at home at Caterham and Sheppard at his home at Catford. Ramsay stipulated that he should inspect these home laboratories once a year ('a very pleasant and memorable event') and that Sheppard and Mees should continue as internal students, reporting periodically on the progress of their work. They each spent about two hundred pounds on their equipment.

The work of the next three years led to the theses for the D.Sc. degree. The investigations were first concentrated in the field of sensitometry, and were extended to include the statics and kinetics of development, including the chemistry of the ferrous oxalate developer, the microscopic structure of the developed image, the theory of fixation, and the effect of oxidizing agents on the latent image. Eleven papers were published between 1904 and 1907 by the Royal Photographic Society, the Chemical Society and the Royal Society. They formed the basis for the doctorates and for a book which is still a classic in its field, *Investigations on the theory of the photographic process* (known as 'Sheppard and Mees').

Sheppard was awarded the 1851 Exhibition scholarship and went to the continent. Mees had always intended to be a teacher—not a professor but a schoolmaster. It seemed to him that English schools should give more weight to the teaching of science, the greatest teachers at that time being in the field of the classics. His reasoning was that, if a man devoted to research would become a schoolmaster, he might influence some of the more promising students to go into science instead of following the teachers of the classics or mathematics. Mees was devoted to experimental science and confident of his ability to inspire other people. He felt that with the prestige of his doctorate he should be able to get a good position as a schoolmaster, and perhaps ultimately a headmastership.

Ramsay, however, had other views. He was convinced that there was a great need for scientific men in British industry, and that a student who showed ability for research in a practical field should go into industry. He, therefore, urged Mees very strongly to try to get an industrial position, and, in view of Mees's experience and competence in the field of photographic research, he suggested the photographic industry.

Even before he went to University College, Mees had felt that one purpose for being on earth was to increase knowledge, and that it was well worth while for a man to study science. When he was about twenty years old, he began to be conscious of the possible applications of science to economics. He was impressed by the study of the economics of London which had been made by Charles Booth, became a Socialist, began to meet other Socialists, and joined the Fabian Society, partly as a result of the influence of H. G. Wells. At that time he was, however, reading the writings of Francis Bacon and became impressed by two of Bacon's ideas, namely, that 'knowledge comes from observation and experiment, and the other, that the science that people were acquiring might be applied to increase the wealth of man'. These ideas struck him very strongly, and he realized that the important thing was the acquisition of knowledge, and the next most important thing was the application of knowledge to industry, so that the poverty in the country could be relieved. It seemed to him that by applying science to industry he could help increase the total amount of wealth, rather than redistribute it. He then gave up Socialism, and for the rest of his life devoted himself to experimenting on a large scale on the application of scientific research to the problems of industry, and specifically to an attack on the problems of photographic science.

Ramsay had convinced Mees that he should go to the photographic industry, so he went to a friend, S. H. Wratten, who was the active manager of the old but small private firm of Wratten and Wainwright, at Croydon, and asked if he would give him a job. He wanted to do scientific research with a view to improving photographic products and processes. The result was that Wratten and his father incorporated the company, gave Mees twenty-five per cent of the stock for £1250, and made him joint managing director. He started on 3 April 1906.

One of the first things that Mees did at Wratten and Wainwright was to work on colour sensitizing the Wratten plates by means of 'sensitizing dyes'. Such dyes had been applied to plates by means of a bathing technique by the Farbwerke Hoechst (Meister, Lucius and Brüning) in Germany, and the English agent of the firm suggested to Wratten that he should make plates sensitized in the same manner, because some English customers were complaining. When the problem was put to Mees, he realized that the proper thing to do was to incorporate the dye in the emulsion before coating on the glass, and this resulted in the production of plates which were superior to the German plates. Improvement in the plates, particularly by use of the dyes Pinachrome and Pinacyanol, resulted in the marketing of the 'Wratten Panchromatic Plates' which were a great success almost at once, putting the Wratten and Wainwright firm into a leading position as the source of supply of colour-sensitive plates. Mees found in Germany a new dye called 'Filter Yellow K' with which he made light filters which became the 'Wratten K

Filters'. Tartrazine had been used previously but it had a transmission in the near ultraviolet and its absorption was not very sharp. The new dye resulted in the production of the yellow filters which have been standard ever since.

With the increasing use of panchromatic plates, a large number of other colour filters became necessary, particularly for the photomechanical reproduction field, and, with A. J. Newton of the Bolt Court School of Engraving and Lithography, Mees worked out a standard set of tricolour filters which have been well known since as Wratten A, B, and C tricolour filters. A visit to Hoechst yielded a large number of dyes which were not generally available and Mees made a systematic study of their absorption spectra, and published them in *An atlas of absorption spectra*, which was put out in London in 1909. This study formed the basis for the development of a wide range of light filters.

The spectra were photographed in a wedge spectrograph which was a small diffraction-grating spectrograph, over the slit of which was fitted a wedge of black glass which varied the intensity from one side of the spectrum to the other. The spectrum was photographed on panchromatic plates in the instrument and showed the colour sensitivity curves of the plates. The instrument was later marketed by Adam Hilger. The absorption spectra of dyes could be photographed by placing a wedge-shaped cell containing the dye solution over the slit.

In addition to the range of Wratten filters, a group of 'safelights' were developed, which were essentially light filters used over lamphouses, to provide in darkrooms illumination by which the products of varying spectral sensitivity could be handled safely.

The Wratten and Wainwright Company had very little money for advertising, so Mees put out technical booklets which would give people information on how to use photographic materials. Instead of advertising the materials, he advertised the booklets. Mees was thus a pioneer in the publication of what are now known as data books, instruction manuals, and technical booklets.

Mees had a certain amount of time to do research, although it was technological rather than scientific. He worked, for instance, on the Lumière Autochrome Plate, which had been put on the market in 1907, and made a study of materials for direct-colour photography. In 1908 he opened up a new field of photographic research with the study of the resolving power of photographic materials, as a result of which the first paper on this subject was published in the *Proceedings of the Royal Society*. This study resulted in the development of plates of increased resolving power relative to their speed, and these were put on the market as 'Process Panchromatic Plates' which were widely used for preparing colour-separation negatives from coloured originals for making halftone printing blocks. The new panchromatic plates were of interest not only in commercial photography and photoengraving but also in spectroscopy and astronomy, and at that time Mees formed some of the connexions with these fields which he continued for the rest of his life. At the end of two years he had put out a long line of new photographic products, most of which were eminently suited for use in scientific work, especially in spectroscopy and astronomy.

In 1909 Mees visited the United States to assist the American Bank Note Company as a consultant on problems relating to counterfeit banknotes. During this time he visited George Eastman, the founder of the Eastman Kodak Company at Rochester, N.Y., and saw the Kodak Park Works. In 1910 he was consultant to the Cotton Powder Company, of Faversham, concerning the stability of nitro-cotton explosives. In 1911, Nobel's Explosives Limited used his consulting services in connexion with the stability of cordite. At the beginning of 1912 Mees received a telephone message from Thacher Clarke of Kodak Ltd. Clarke was Eastman's personal assistant and correspondent in Europe, and he told Mees that Eastman would like to repay the visit he had made him, by visiting the Wratten and Wainwright factory in Croydon. There Mr Eastman was shown the factory and the work they were doing on panchromatic plates and colour filters. Eastman had already come to the conclusion that, although a good deal of experimental work had always been done by his company in the United States, a separate scientific research laboratory was needed which would devote itself to the fundamental theory of photography. Eastman told Mees he would like him to go to Rochester, and Mees agreed on the condition that Eastman bought the Wratten company. Eastman was agreeable, arrangements were made for the incorporation of the Wratten company into Kodak Ltd in England, and Mees went to the United States in April, 1912, to work out plans for the Kodak Research Laboratory. Under his guidance, it was to become one of the world's great industrial research laboratories. The first members of the staff started work in the autumn of 1912.

At the beginning of 1913 the laboratory had a staff of about twenty people. It now numbers well over a thousand. Mees's old associate at University College, S. E. Sheppard, had joined the organization and later became assistant director. P. G. Nutting was the first physicist. He came from the Bureau of Standards where he had been in charge of the Division of Optics. When he left in 1917 to become director of the new research laboratory of the Westinghouse Electric Company, his place was taken by L. A. Jones, also from the Bureau of Standards. In 1913, J. G. Capstaff came from Newcastle and J. I. Crabtree from Manchester to work in photographic research. A. S. McDaniel, also from the Bureau of Standards, was put in charge of inorganic chemistry. Hans T. Clarke, the organic chemist, joined in 1914, and F. E. Ross, the astronomer, in 1915. Mees chose his staff well from the start.

The organization was not under the control of the management of the factory, and it was not intended that it should concern itself primarily with manufacturing problems. Mees pointed out that, although industrial laboratories usually deal mainly with the application of science rather than with the creation of new knowledge, in the case of photography there were no general academic sources for the advancement of the theory of the process. The research laboratory was, therefore, developed as a research institute devoted to the advancement of scientific knowledge as well as to its application to the technology of photography. From the beginning the staff was engaged in the elucidation of the theory of the photographic process. Later, of course, much effort was also spent on the practice of photography and its application to science and technology, as well as on improvements in manufacture and methods of testing and control.

His position in the new laboratory enabled Mees to realize his dream of experimenting on a large scale with the application of scientific research to the problems and ambitions of industry. He admitted that his experience as a research organizer and director were confined mainly to photographic research and to the photographic industry, but he had close connexions with other progressive directors of research in industry, including W. Rintoul, of Nobel's Explosives Ltd, and W. R. Whitney of the General Electric Company in the United States. From Rintoul he learned much about the organization of industrial research, and from Whitney ('Whitney's laboratory was not so much organized as inspired') he learned the dangers of over-organization.

In the Kodak laboratory Mees attempted to use a certain amount of formal organization but avoided over-organization, and as the years went on the lessons that he had learned were embodied in a book, *The organization of industrial research*, published in 1920, and republished in conjunction with J. A. Leermakers in a revised and greatly enlarged form in 1950. He grouped laboratories according to whether the problems investigated were connected with one common subject or whether they were of many kinds having no connecting bond of interest. The first type of laboratory might be called 'convergent' and the second 'divergent.' His new laboratory had been organized in departments and was of the 'convergent' type; that is, the work of all the departments was interconnected so that they converged on the scientific foundations of photography and on their applications in the broadest sense. In more recent years the work of the laboratory became more divergent, because it was inevitable that discoveries were made which might have commercial importance and which were not directly related to the original main purpose of the laboratory. One of the early policies established was that the scientific work was to be published in the scientific journals, and another was that the whole laboratory should be open to the men in it, and a conference system was organized whereby the whole staff could know about all the work going on.

Mees had little use for the 'project system' where committees decide what shall be done, how much shall be spent, and when the work shall be dropped. He felt that the man who best knew the direction the work should take was the man who was doing it, and he tried to choose his men so that he could rely on them from this point of view. It is appropriate to quote from one of Mees's talks on research, for it typifies his attitude: 'Research is a gamble.

It cannot be conducted according to the rules of efficiency engineering. Research must be lavish of ideas, money and time. The best advice is, don't quit easily. Don't trust anyone's judgement but your own, especially don't take any advice from any commercial person or financial expert and finally, if you really don't know what to do, match for it. The best person to decide what research work shall be done is the man who is doing the research. The next best is the head of the department. After that you leave the field of best persons and meet increasingly worse groups. The first of these is the research director, who is probably wrong more than half the time. Then comes a committee, which is wrong most of the time. Finally there is a committee of company vice presidents, which is wrong all the time.'

Sensitometry was always one of the major research fields, particularly in relation to the theory of tone reproduction, which is the study of how far photographic reproduction would resemble the original in the distribution of brightness attained, and the prediction from the sensitometric characteristics of photographic materials of their ability to reproduce tone values with fidelity. The first work was an investigation of the sensitometry of photographic papers, and this led to the general study of tone reproduction undertaken by L. A. Jones and his colleagues. This work and its corollaries essentially completed that started in 1890 by Hurter and Driffield, and the whole subject was summarized in Mees's Hurter and Driffield Memorial Lecture before the Royal Photographic Society in 1924 under the title 'The photographic reproduction of tone.' This work was continued by Jones in many important papers, and is still a subject of active investigation.

Mees's early work on resolving power was continued actively, and in 1924 F. E. Ross published a monograph, *The physics of the developed photographic image*, in which the subject as a whole was reviewed. The work of Ross was continued by O. Sandvik and G. Silberstein. The subject of granularity, or its subjective aspect of graininess, was actively studied, particularly by N. Deisch, A. C. Hardy, E. M. Lowry, E. W. H. Selwyn, and G. C. Higgins.

The sensitivity of the silver halide grains, the mechanism of the action of light and the nature of the latent image have been the subject of investigation by many laboratories, but the research laboratory at Rochester played a very significant part. About 1920, when the subject was also under investigation in England and Sweden, the very difficult problems involved in measuring the numbers of silver bromide grains of different sizes in film emulsions were attacked, and it was decided that the only satisfactory method of measuring the size distribution was by photomicrography, measuring the actual photographs of the grains at a magnification of 2000, and classifying them according to their sizes. Great numbers of size-frequency curves were obtained in an attempt to correlate the properties of the emulsion in terms of grain size and size frequency.

In the first five years at the laboratory, Sheppard's time was spent primarily on a study of the physico-chemical properties of gelatin, which is the medium in which sensitive photographic emulsions are prepared. This led to a great number of papers on the subject, and it culminated in 1929 in the establishment of a procedure for making a standard gelatin by methods easily reproduced in the laboratory, and a standard de-ashed gelatin was sold to biological and chemical laboratories. A series of papers was published in 1930 on the structure of sols and gels which clarified the difference between these systems and ordinary solutions. The well-known sensitizing property of gelatin in the photographic process led to a systematic study of the difference between gelatins in their sensitizing power and the nature of the substance in the gelatin which confers sensitivity. By a painstaking series of experiments it was found that the sensitizer was concentrated in the liquors obtained by the acid treatment of the raw materials left after liming the hides, and it was eventually identified as allylthiourea. This was perhaps the greatest advance made in Sheppard's scientific career, and all further studies of the photographic properties of gelatin, the nature of sensitivity, and the latent image have been conditioned by it.

Other work at this time in Sheppard's branch of the laboratory covered a great range, including photovoltaic effects—that is, the electrical response of silver halide to light—the colloidal structure of the materials used for film support, the nature of development, and in particular the nature of dye sensitizing, the adsorption of sensitizing dyes to silver halides, and the structure of the adsorbed layers and their sensitizing effects. It was evident that dye sensitizing would involve not only the adsorption of the dye to the silver halide lattice, but an understanding of the nature of the absorption of light through the resonance structure of the dye. The absorption spectra of dyes in various solvents, in vapour phases, and when adsorbed to surfaces, were therefore investigated.

The work of Sheppard and others suggested that the sensitivity of the silver halide grains depends upon the existence of submicroscopic specks consisting of silver sulphide (with possibly also some silver), and that these specks act as nucleation centres for the material produced by the action of light (probably silver) which, in turn, initiates development. Sheppard's discovery of the sensitizing effect of allylthiourea indicated it could be the origin of the formation of the silver sulphide, and he and his colleagues advanced a theory of the action of light which they called the 'concentration speck theory'. It was suggested that the specks of silver sulphide, perhaps with silver, produced a strain in the crystal lattice, and that when light fell on the crystal it liberated photo-electrons which were transferred through the crystal until they reached the speck, where metallic silver was set free. This theory essentially provides the basis for later work by the physicists J. H. Webb of the Kodak Research Laboratories and by R. W. Gurney and N. F. Mott of Bristol, who applied the methods of quantum mechanics to explain latent image formation. Work done in Mees's laboratories on reciprocity failure also threw much light on the understanding of the formation of the latent image.

Work was done on the mechanism of development, partly by studying the structure of the developed silver by the electron microscope. A photographic developer is a reducing agent and it discriminates between exposed and unexposed silver halide by virtue of the silver specks of the latent image catalyzing the reduction of the exposed grains. The developing agent is adsorbed to the silver bromide and forms a complex which breaks down to silver in the presence of the silver nuclei, a layer of electrically charged bromide ions surrounding the silver bromide crystals playing an important part in controlling the initiation of development. Much study was devoted, and still is, in the laboratory, to the reactions occurring in development and the kinetics of the reactions, both for black-and-white and for colour photography, where the oxidized developer produced during development produces dyes by reaction with couplers.

For the first five years of the laboratory, work was devoted almost entirely to the theory of photography, and it was not until 1919 that the development of new, commercial products began to assume real importance. In that year, work was started on the development of a system of amateur cinematography using narrow film. Capstaff, in particular, steered the work and followed it for many years. It was decided to process the film in the Kodak Company's own stations, and to develop simple cameras and projectors. It was also decided to use the reversal process, in which a negative is developed, the silver image is dissolved out, and the remaining undeveloped silver halide is then exposed and developed to a positive silver picture. Capstaff conceived the idea of controlling the exposure of the residual silver halide according to the amount that was left in the film after bleaching. This was a very important step in processing the film, compensating as it did for variations in exposure. The so-called 'Cine Kodak Process' was put on the market in 1923. (Probably the first feature film to be made on the narrow-width film was by members of the laboratory staff. It was called 'Out of the fog' and was a skit showing Mees landing on the shore of Lake Ontario near Rochester with his contingent of Britishers to start the laboratory.) A development from this was the Kodacolor system of amateur cinematography in colours, which was derived from an invention of R. Berthon and which was made available to the public in 1928. It was an additive colour process using lenticular film. Amateur cinematography in colour continued to be done by this process until the new Kodachrome film was put out in 1935.

From the beginning, Mees had set as an objective the working out of an amateur colour photographic process. Before he went to Kodak he had been interested in the so-called additive-screen plate processes and continued his work in Rochester, but it was eventually abandoned. The so-called subtractive colour processes at that time were difficult to do, and in 1914 Capstaff started work on a two-colour subtractive process particularly for portraiture. Results were actually shown under the name of 'Kodachrome' in 1915 at the San Francisco Exhibition, but the process never really reached commercial acceptance. The work, however, was carried on experimentally for the professional motion picture field, although eventually it was essentially replaced by the three-colour imbibition process of the Technicolor Motion Picture Corporation, and later that was largely displaced by the present negative-positive colour films.

Ever since the early days at Croydon, starting with the preparation of the Wratten panchromatic plates, Mees had been interested in sensitizing dyes for making plates and films respond to all the visible spectrum. During the war of 1914-1918, when the only source of such dyes (Germany) was cut off, Mees started H. T. Clarke on their preparation in Rochester. Following successful preparation of the early dyes, attempts were later made to make new ones and a prolonged investigation was started. It was not until 1928 that it was realized in the Kodak laboratories that heterocyclic bases, other than those of the quinoline series, could be used for making polymethine dyes similar to the cyanines of which the early sensitizers were examples. A few of the newer cyanine dyes had been made by W. H. Mills and his associates in Sir William Pope's laboratory at Cambridge, but they had not been applied widely to the commercial production of photographic materials. In 1928, a number of dyes were made in Rochester from benzotriazole which proved to be very satisfactory sensitizers for the green and orange portions of the spectrum, and following these, dyes from the naphthathiazoles proved to be the best sensitizers for the red discovered up to that time. This work was under L. G. S. Brooker who had been at King's College, London, and who first worked with Clarke on sensitizing dyes in Rochester. After this, the organic chemists made several hundred new sensitizers a year and Mees established the system for evaluating them. Out of this work has come a great variety of photographic materials for special purposes. The sensitivity of films to tungsten filament lamps was greatly increased and new sensitizers increased the speed of fine-grained emulsions to a greater extent than that of coarse-grained emulsions; this was very largely responsible for the big increase in the use of miniature cameras. It was not long before most of the negative materials, and many of the photographic printing papers used commercially, employed the new sensitizing dyes which resulted from the extensive investigation prompted by Mees.

About 1920, Mees came into contact with Leopold D. Mannes and Leo Godowsky, two young men who were musicians by profession but who were enthusiastic amateur experimenters in colour photography, and for them he made experimental coatings for their work. Mees had run into a number of dishonest 'inventors' in the colour field, and was much impressed by the sincerity of these men. As a result of the availability of new sensitizing dyes, it seemed probable in 1930 that a process on which Mannes and Godowsky were working might be developed into a practical method of colour photography, but it was clear that close co-operation would be needed with the research laboratory and, especially, with the emulsion research group. Mannes and Godowsky were, therefore, invited to join Mees's laboratory staff and to continue their work. The new Kodachrome process was developed and the first Kodachrome film was put on the market in 1935 as 16mm amateur cinematograph film. From this work developed a system of making colour prints on white support by the Kodachrome process, and a negative-positive process which was introduced under the name of Kodacolor; later a variety of other related colour systems were evolved. Colour photography became a large and important part of the photographic industry.

The work on sensitizers and parallel work on other aspects of the technology of preparing photographic emulsions made it possible to improve photographic materials for scientific work, notably in astronomy and spectroscopy. Mees personally became very interested in astronomy, and was directly responsible for some of the great progress made in astronomy in the last thirty years. He undertook to work out new plates for astronomical study. In 1912, while Mees was still at Wratten and Wainwright, but after he had agreed to come to Rochester, he had had a visit from F. H. Seares of Mount Wilson Observatory. Seares told him that G. E. Hale had asked him to invite Mees to go to Mount Wilson and install a laboratory for making plates specially for astronomical work, because he believed that considerable improvement could be made. Mees told Seares that he had already agreed to come to Rochester to organize the research work of the Eastman Kodak Company, but promised that as soon as possible he would make special plates in Rochester for the astronomers. Finally, from 1933 on, in a new emulsion research laboratory which Mees had established in 1931 under C. J. Staud (later Mees's successor as director of the Kodak Research Laboratories), he was able to develop a large series of plates sensitive to a wide range of spectral bands in the visible and near infra-red, and having many types of sensitivity, contrast, and graininess. These were of great value to the observatories, and Mees was able to redeem the promise that he had made to Seares. The work was recognized by the award of the Henry Draper Medal of the National Academy of Sciences.

The establishment of the emulsion research laboratory under Staud in 1931 represented an enlightened departure from tradition in the photographic research field, for emulsion making is the main prop of the photographic industry, and its secrets are carefully guarded. Some technological rather than scientific work was done in the emulsion-making department which was established as part of the original laboratory, and some physico-chemical studies had been made on the properties of emulsions in the chemistry department. Little progress was made in the application of science to emulsion-making methods, however, until the special laboratory was founded in 1931 and experimental work was started to cover systematically all phases of the subject. These included the synthesis of sensitizing dyes, the relation of their structure to their properties, and the mechanism by which they sensitize; study of nonsensitizing dyes for photographic purposes, such as protection against halation; chemicals to be added to emulsions to give them special properties, including antifoggants, preservatives for ensuring good keeping properties, and hardeners; methods of chemical sensitizing derived from the original work of Sheppard on sulphur compounds; the special problems of colour photography; and, particularly, the technology of emulsion making. All this work has contributed to the production of new photographic materials of a great range of properties and uses.

Important results for the graphic reproduction fields have come from the special departments which Mees established to study the problems involved in the reproduction of photographs by the various printing processes, and in copying documents for disseminating the information in them rapidly, cheaply, and accurately, for reducing the storage space, for security, and for effectively increasing their life. The photographic aspects of sound recording were studied systematically after 1932 and the results were made available to the motion picture industry. When the laboratory was first established, a room was equipped for experiments in radiography, and X-ray research has been an important function ever since, embracing the sensitometry of X-ray materials, the properties of intensifying screens, the reduction of scattered radiation, improvements in medical and industrial radiographic technique, soft X-ray radiography, X-ray motion pictures, and X-ray crystal analysis.

The characteristics of a photograph depend on the kind of emulsion and on the nature of development. The two are closely interdependent. Mees had studied the laws of development since his start in research, and when he founded the laboratory he included a department for investigating the practical aspects of development and working out formulae for developers, and for studying the other aspects of 'processing,' which includes fixing, washing, drying, stabilizing, bleaching, intensifying, toning, and so on. The work was essentially of a practical nature, and was extended to include the design of apparatus in which processing could be performed on a continuous, large-scale basis.

During the World War of 1914-1918, starting particularly in April 1917 when the United States declared war on Germany, Mees devoted the efforts of his laboratory largely to military problems. Work was done on the production of graticules, on colloidal fuels of pulverized coal for the Navy, on aerial photography (in particular the penetration of haze), on the camouflage of ships at sea, and a variety of other subjects. In March 1918 a school of aerial photography was started in Rochester for the Air Corps, using the laboratory staff as instructors. The course was planned in collaboration with the military authorities, but especially with Captain Harry Wilsden, of the Royal Flying Corps. A total of 1995 men was graduated. The laboratory's efforts were paralleled on a far larger scale in the war of 1939-1945, and its facilities and knowledge have since been used to a large extent by the military people. Mees himself devoted much of his own time to some of the important problems of the last war, and was on top-level committees. In 1946, he showed the writer with great pride a gold cigarette case with King George VI's signature engraved on it, but omitted to say what he got it for.

In 1918, there was in the United States a serious lack of synthetic organic chemicals, the supply from Germany, which was almost the exclusive source, having been cut off in 1914. With Eastman's support, Mees organized a department of synthetic organic chemistry under Hans T. Clarke to supply special chemicals to research laboratories and universities. This department developed into one of the major sources of special organic chemicals for research. At the start, only 265 chemicals could be collected and made available. By 1921, 1144 chemicals were listed, of which the majority had been synthesized, and eventually over 3500 chemicals were produced. The question of research in fields external to the main purpose of a laboratory presents problems to the director of research in industry. It is easy to be diverted too much from the main work of the laboratory, but there will always be some inventions or results of research which will repay development. Mees had to make decisions as to what should be done in his laboratory outside the photographic field. The matter of the synthetic organic chemicals was one, as were also some of the special subjects worked on for the war efforts. But there were many others.

Following his work on colloidal fuel, Sheppard studied the electrodeposition of rubber at an anode from electrically charged rubber-latex particles. To the latex suspension were added other agents required in rubber manufacture, such as the sulphur, accelerator, and filler, and the electrodeposition was used for the production of inner tubes, gloves, rubber-covered wire, and other such items. A similar process had been worked out about the same time in Hungary, and a commercial firm was organized with the Goodrich Rubber Company in the United States in which the Hungarian and Kodak patents were incorporated; the resulting company, The Anode Rubber Company, has been practising production of sheet rubber materials since that time.

In 1940, the research laboratories, at the request of H. C. Urey, undertook the task of separating the stable isotopes of nitrogen, in particular the heavy isotope ^{15}N, which was needed as a tracer in biological and chemical research. The isotope was concentrated utilizing chemical exchange between ammonia and a water solution of ammonium nitrate in packed fractionating columns of very great length, cycling over periods of many months. The ^{15}N was made available commercially as nitric acid and potassium phthalimide and, in 1946, the production of ^{13}C, the heavy isotope of carbon, was started, using chemical exchange between hydrogen cyanide and a solution of sodium cyanide. The ^{13}C was sold as methanol and methyl iodide.

In 1914, the Eastman Kodak Company began to manufacture the lenses for its own cameras, using conventional optical glasses, but the computing staff eventually became interested in obtaining improved optical glasses, and discussed the matter with G. W. Morey who had done work of great value during the war in the production of optical glasses for military purposes. The main objective was to make glasses of even higher refractive index than the available barium crown glasses but with a low dispersive power. High atomic-number elements were chosen for study in silicate, borate, and phosphate glasses, and by 1933 it had been found that boric oxide was the best fluxing agent, and oxides of the rare-earth elements, lanthanum and thorium, and of other elements, columbium, tantalum, tungsten, titanium, zirconium, and strontium, were used in portions up to 80 per cent by weight. In 1934 glasses had been made with a refractive index of 1·85 and a reciprocal relative dispersion of 43·0. The new rare-element borate glasses were extremely corrosive to all known pot refractories, and platinum pots were therefore used for their actual production. The first commercial glass was delivered in 1939 and, during the war of 1939-1945, 125 000 pounds of rare-element glass were produced, the yield of finished, usable glass being 95 per cent of the theoretical available in the batch. The new glasses were first used in lens design in 1934, and many lenses still contain the high-index glass. Later, unusual flint glasses were worked out in the laboratory, the most useful containing titanium oxide and sodium fluoride in addition to silica. All-fluoride glasses were also designed having the characteristic low refractive index and extremely low dispersion of natural fluoride minerals.

In 1929, K. C. D. Hickman, who had come from the Imperial College of Science, and who had been working on photographic chemistry in the laboratories for a number of years, studied the use of high vacua in certain aspects of the manufacture of film, and this led to an investigation of the operation of condensation pumps and of the properties of high-boiling organic liquids having low vapour pressures. C. R. Burch had suggested the use of high-boiling oil instead of mercury in the pumps, and Hickman found that dibutyl phthalate and, later, dioctyl phthalate, were suitable. Difficulties with the decomposition of the organic compounds led to the design of multistage fractionating pumps in which the fluid was continuously rectified and maintained in a condition of high purity. This gave much improved high-vacuum pumps. Use of these pumps for determining vapour pressures turned Hickman's attention to the possibility of fractionating substances by distillation in high vacuum and to the design of molecular stills. Special stills were applied to the distillation of a number of oils, in particular, fish liver oils, in which case it was found that vitamin A could be very considerably concentrated. On the basis of this work a company named Distillation Products Incorporated was organized to exploit the production of vitamin concentrates, particularly vitamins A and E, by molecular distillation. An important improvement in manufacture resulted from Hickman's invention of centrifugal molecular stills. Distillation Products Inc. (now Distillation Products Industries) later manufactured monoglycerides and other oil and fat products in addition to vitamins.

Until recent years, the most suitable substance known for slow-burning film base was cellulose acetate, and the laboratory co-operated with the manufacturing departments in research on the production of cellulose acetate and cellulose acetate derivatives, originally for film base but later for the production of textile yarn and moulding products. In 1937, a special research unit was started to work in this field, which had become the province of the Tennessee Eastman Corporation, and among the important developments were a method of making crêpe acetate fabrics and a large number of dyes for cellulose acetate. Study of the reaction between oxides of nitrogen and cellulose, particularly under W. O. Kenyon in Mees's laboratory, showed that polyanhydroglucoronic acid was formed. Such oxidized cellulose is sold to the pharmaceutical industry for soluble dressings and sponges used in surgery.

Mees frequently announced the most important results of the work of his laboratory by giving special and sometimes popular lectures on the subjects. He was much in demand as a speaker. He was proud of the work and of its contribution to the science of photography, and he recognized its public relations value. He felt that publication of much of the work would enlarge the knowledge of the basis of the subject, and even though it might help his competitors in business the resulting advance in the field as a whole could not fail to benefit his Company and science. A large part of the scientific work was published in the scientific, technical, and photographic journals as 'Communications'. They numbered over two thousand at the time Mees died. Starting with Communication No. 1, in 1913, he published biennial and later annual volumes of shortened versions of the papers as *Abridged Scientific Publications*, intended to concentrate the material in a form not requiring availability to the reader of a wide range of journals. In 1915 he began publication of a *Monthly Abstract Bulletin* covering the photographic and related literature. In the twenties he published a series of six *Monographs on the theory of photography*. Many of his staff have written books. Mees started a research library in 1912 which developed into the most complete photographic library in existence, and now contains over 30 000 volumes and over 450 periodicals.

The research laboratory has been expanded a number of times. In addition to the original building of 1912-1913, a larger and completely new building was erected in 1931 and this was greatly enlarged in 1937 and 1951. The rapidly increasing demands on space for expanding personnel showed that this could not accommodate the requirements for very long, and a new research centre was planned consisting of a group of separate buildings each devoted to a main field of the work. When Mees died, the first building, devoted to physics, was under construction. It was occupied early in 1961. When Mees retired in 1955 the staff numbered 734, and at the present time (June 1961) it counts well over one thousand.

The Eastman Kodak Company has manufacturing plants in several countries, and laboratories were also established at some of them to do research in the science and technology of photography. The English laboratory was established at Harrow in 1928, the first director being Walter Clark (who later went to Rochester, when his place was taken by E. R. Davies); the French laboratory at Vincennes was organized in 1928, the director being Marcel Abribat; there is a laboratory at Abbotsford, Victoria, Australia, under N. B. Lewis; and, in 1946, a tropical research laboratory was started in Panama under C. C. Soper. Other research laboratories of the Company deal with chemical problems. That of the Tennessee Eastman Division at Kingsport, Tennessee, is concerned with plastics, fibres, and chemicals;

that of the Texas Eastman Division in Longview, Texas, with large-scale synthesis of petrochemicals; and that of the Distillation Products Industries Division with vitamins and high-vacuum distillation, especially of fats. All of these laboratories stem from Eastman's early interest in research and his selection of Mees to develop it.

Basically, Mees was a pioneer, energetic, aggressive, and a great and authoritative talker. To some he appeared rather rude but this was partly a form of brusque impatience over intellectual laziness or sloth in others. He always rose to a verbal challenge and could be a devastating opponent in debate. He wanted facts, knew their significance, could sort them out, and spoke with great clarity. He rarely admitted defeat, but was ready to adjust his thinking to the new idea.

Mees recognized the significance of the individual and brought into his sphere as many as he could, and protected them from outside interference. He had an unquestioned talent for keeping his men stimulated. He had vision and forcefulness and an ability to encourage others which led to much of the great progress in photography in this century. He was one of the rare and reassuring people who recognize and encourage genius, leave it alone to blossom, and then steer its product to a worth-while end.

In his book, *Industrial explorers*, Maurice Holland said that Mees was 'earnest in his search for truth and knowledge. He had no use for charlatans, and woe betide the individual with spurious facts who tries to put them over. On such occasions, Mees uses facts as a soldier uses hand grenades, and the result is as devastating.' Once in a while, however, he would express himself with conviction but error on subjects with which he was not too well acquainted. The writer remembers occasions early in the war of 1939-1945 when Mees had daily discussions about allied strategy with his friend Colonel O. N. Solbert, a West Pointer, retired. He was invariably wrong. One of the most glaring instances of his being wrong was when he maintained that the German occupation of Norway was one of their great mistakes, because it would undoubtedly lead to their undoing at the merciless hands of the British fleet. He was, however, deeply concerned with the fate of Britain early in the war, and he was moved to tears in contemplation of the possibility of a German invasion of the British Isles.

Mees had always been an avid and lightning reader, and it was fascinating to see him skim a page and hurl the essence of it at his listener who had scarcely got beyond the first line. He had a very extensive library and loved printing as a means of communication. He was well read in Egyptian archaeology and well versed, partly by reading and partly by experience, in astronomy and marine biology. Late in his career he went whole-heartedly into a programme for doing low-cost, short-run colour printing, primarily to enable scientists to illustrate their articles in colour—a thing which they could not afford to do adequately by the normal methods. He also worked hard to get the printing industry in the United States to inaugurate a co-operative research programme, but with little success.

Mees himself wrote about 250 articles, most of them in the form of scientific papers, and a number of important books. In 1942, he made his greatest contribution to scientific literature with the publication of *The theory of the photographic process*—'a fat and heavy book'—written with the aid of his associates. In it was brought together the state of knowledge of the theory of photography, and it was intended to form a comprehensive survey having the object of stimulating work on photographic theory. A second edition was published 12 years later, entirely rewritten, and with about one-third of its contents changed. He hoped that members of the research laboratory which he founded would put out revised editions from time to time with the same objective.

Mees's accomplishments have been recognized by a great array of honorary degrees, invited lectures, and honours and awards given him by a great number of societies and organizations in many countries. His greatest recognition was the award of the Fellowship of the Royal Society in 1939. He was also made a Fellow of the American Philosophical Society in 1937, and a member of the National Academy of Sciences in 1950. An honour of which he was particularly proud was the award in 1913 of the Progress Medal of the Royal Photographic Society, the highest honour in photography, for 'his researches, discoveries, and publications in the physics and chemistry of photography'. He received the same award again, and for the same reasons, in 1953 (Sir W. de W. Abney was the only other recipient of the Progress Medal on two occasions). He was the only non-astronomer to receive the Henry Draper Medal. In 1954, he received the gold Franklin Medal of the Franklin Institute. He was the Children's Christmas Lecturer at the Royal Institution in 1935 on 'Photography,' and the Children's Christmas Lecturer for Young People at the Franklin Institute in 1941, on 'Colour photography'.

Mees admitted that he was never particularly interested in outdoor sports, although he played some golf, but even this had to be dropped when he had a leg amputated when he was about 70 years old, a matter which he endured with typical fortitude. In this connexion, on 21 May 1951, he wrote a letter: 'To my friends: I was quite unexpectedly stricken with a serious illness, and I thought I would like to write to all of you and tell you what has happened to me, what I am hoping to do now, and especially how much I appreciate your loving kindness.' He then related how on 29 April, returning to Rochester after giving the Haynes Foundation lectures at the California Institute of Technology and from the National Academy meeting in Washington, he suddenly developed a complete block of the larger artery, leaving the left leg bloodless. Ten days later they took off the leg. 'The prognosis is quite good. I shall be able to use an artificial leg and shall be handicapped very little. I owe everything at the moment, of course, to the skill of the doctors and the surgeon.' It was quite evident to his friends, however, that the loss of his leg inconvenienced him more than was evident from his general bearing, although he did continue to drive his car, and appeared to evince his early spirit. His greatest personal loss occurred with the death in 1954 of Mrs

Mees, with whom he had lived a close and devoted life since their marriage.

Mees had retired as active director of research for the Eastman Kodak Company in 1947, but he continued as Vice President in charge of research for eight more years, finally retiring in 1955. He died in Hawaii on 15 August 1960, at the age of 78.

In earlier years, Mees had spent a lot of time in Bermuda, but in later years he transferred his travels to Hawaii, where he had family ties, and he retired to these Pacific islands in 1955. After going there, he did not return to the United States, but devoted himself to many local interests. He retained his interest in astronomy and had a big telescope, and also resumed the interest in marine biology he had fostered in Bermuda. He cultivated orchids, lectured at the university and schools there, wrote the major part of the manuscript of a book on the story of Kodak photographic research, and, at his death, was working on an autobiography. He had a constant parade of visitors with whom he could talk in his old, enthusiastic way, and received correspondence from all over the world. It is said that he lived an idyllic life, in a peaceful spot overlooking the Pacific Ocean. He had a batman, a cook, and his secretary who lived nearby and who had been with him a great many years in Rochester. He kept himself busy, yet it is felt that he was not particularly happy, especially with the loss of his wife and his physical handicap.

Mees married Alice Crisp on 1 June 1909, at Croydon. Her father was Isaac Crisp, a reader for Longman's, Green and Co., and her mother was Catherine Elizabeth Watkins. Mees is survived by his two children—Graham Charles, born 11 July 1910, and living in Rochester, New York, and Doris Margaret, born 22 May 1912, now Mrs Rohan S. Sturdy, and living in England.

C. E. K. Mees had led a full life and left a lasting impression as a result of his astonishing breadth of knowledge, his kindliness, his deeply human qualities, his forceful personality, and his extraordinary command of the language and power of expression.

L. A. Jones, one of Mees's associates for the greater part of the time he was in the research laboratories, said of him: 'While he has been successful as an individual research worker, his great success and his great contribution lie in the ability and sagacity he has shown as director. His unbounded enthusiasm, his vivid imagination, and his uncanny ability to suggest the most fruitful experiments to perform have been a constant source of inspiration and encouragement to the members of his staff. His pre-eminent fairness to those working under him, his kindliness, and his humanity have endeared him to the hearts of his associates.'

WALTER CLARK

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David R. Pye.

DAVID RANDALL PYE

1886-1960

David Randall Pye, who died on 20 February 1960, was a scientist and an engineer, although he spent most of his life in government and university administration to which he was called at critical times. From the age of thirty-nine to fifty-seven he devoted himself to building up the scientific research organization of the Air Ministry, at a time when aeronautical developments were so vital to our country's survival; from fifty-seven until his retirement at sixty-five, he undertook the task, as Provost, of re-establishing University College, London, which had suffered severe blows from the enemy's air attack. Had there been no war he would almost certainly have spent his life in engineering science, probably in a university, and would have made many more original contributions of the same high quality as those of his earlier years. But although he thus sacrificed his personal research for his country's needs, he undoubtedly did so partly because of his intense interest in human affairs and in people. He was not by any means a born administrator, or an ambitious organizer, but his wide talents, simple integrity and high standards, enabled him to achieve real success in the administrative work he undertook.

Pye was born on 29 April 1886 at Hampstead, London. His early life was spent in the atmosphere of a country home not far from London where he lived with his parents, three brothers and three sisters, David being the sixth in the family. He was a descendant of John Pye of the Mynde, County Hereford, and Anne, daughter of Roger Andrews of Hereford, through their son, Walter Pye of Kilpeck Castle. Another branch of the family, the Edmund Pyes, lived at Blyth, Nottinghamshire, one of whom had a grant of arms from Sir Richard St George, Clarenceux King at Arms, 2 March 1633-1634. His grandfather, Kellow John Pye, was a Vice-President of the Royal Academy of Music, a composer of well-known church and secular music and for a time organist at Exeter Cathedral. David Pye's uncle Walter was a distinguished surgeon at St Mary's Hospital, London, his book, *Surgical handicraft*, still being republished as a standard work. It is of interest that David Pye confessed to an early ambition to become a surgeon, a career to which his combination of intellect and considerable manual dexterity might have made him well suited.

David Pye's eldest sister, Edith Mary, was one of the few women Chevaliers of the Legion of Honour, an order awarded in recognition of her work in France during the 1914-1918 war. Another sister, Anna Sybil, was an artist bookbinder of repute, and examples of her original work have been included in various public and private collections and exhibited in most of the European capitals and in North and South America. Thus the many interests of David Pye and the width and quality of his mind are clearly related to his family history.

At the age of ten David went to Horton Hall Preparatory School, Northampton. When fourteen years of age he went to Tonbridge School, with an entrance scholarship and a house scholarship, both in classics. After spending one year in the classical sixth he transferred to the scientific side and in 1904, after declining a scholarship to Downing College gained an entrance scholarship to Trinity College, Cambridge. In his biographical notes, Pye says that his first view of science beyond the school curriculum was afforded him by Charles Seargent, a Ph.D. of Marburg University, with whom he worked in his last year at school and went to Germany in the summer of 1904. It is interesting to note that, as is not uncommon with men who later become outstanding in science or engineering, Pye started on the classics side at school.

At Trinity he read for the Mechanical Sciences Tripos, taking the second and third year courses in his first two years and special reading in his third year, and gaining a First Class Honours degree in 1908. He also won a half-blue for rifle shooting. During his last year at Cambridge he worked as a research assistant under Bertram Hopkinson, and also spent a short period at the works of Mather & Platt in Salford, Manchester, gaining practical experience.

In 1909 C. F. Jenkin, F.R.S., was appointed the first Professor of Engineering Science at Oxford University. Pye moved over from Cambridge to join Jenkin, the two of them at first forming the entire staff of the department and being responsible for planning and carrying through a full engineering science course. Owing to this limitation of staff major subjects could be taken only in alternate years, and the teaching work must have called for wide knowledge of engineering science. Naturally Pye became interested in Jenkin's work on properties of materials and his first research was an investigation into the mechanical properties of hard-drawn copper. But he also did research on the properties of carbon dioxide at low temperatures, and it was in the field of thermodynamics and internal combustion engines that he later made his main contributions. During the 1914-1918 war he worked as an experimental officer in the Royal Flying Corps at the Experimental Stations at Orfordness and Martlesham Heath, on experimental design and testing work on aircraft armaments, and later on aircraft performance testing. He also learnt to fly as a pilot. At the end of the war he returned to Cambridge as a Lecturer in Engineering and a Fellow of Trinity College, and along with Henry Tizard, was immediately retained by Messrs Ricardo & Co. as consultant on combustion and in connexion with research on fuels for internal combustion engines. The results of this work were published in the Empire Motor Fuels Committee Report. The trio, Ricardo, Tizard, and Pye, became well known for many contributions to research and development of the internal combustion engine.

In the years 1920 to 1925 Pye made his major individual contributions to scientific research. Three of his most important papers were published jointly with Tizard and contributed much to our understanding of the ignition and combustion processes in the spark ignition piston engine and in the compression ignition engine. At this time there was considerable discussion as to whether the combustion process might be a limiting factor in the development of the internal combustion engine, and the work of Pye and Tizard showed that the propagation of the flame front is rapid enough for the highest engine speeds. With Ricardo and, at a later stage, Egerton, Tizard and Pye put this country ahead in internal combustion research, and led the way to the solution of the problem of knock which was a limiting factor in increasing engine power. In 1925 he left Cambridge to become Deputy Director of Scientific Research at the Air Ministry under Dr H. E. Wimperis, who had just been made head of the new Directorate of Scientific Research. In 1937 Wimperis retired and Pye succeeded him as Director, a post which he held in the early war years until he resigned in 1943. He was appointed C.B. in 1937 and in the same year he was elected to the Royal Society. In addition to his work as Deputy Director of Research, Pye continued to take an interest in academic work and in engineering developments, and it was in this period that he published his well-known books on *The internal combustion engine* in the Oxford Engineering Science Series, of which he became an editor, volume I appearing in 1931 and volume 2 in 1934. These books gave a comprehensive account of the piston aero-engine and have stood the test of time to remain some of the best writings on the subject. They show much independence of thought and are a beautifully clear account of the complex internal combustion aero-engine. At this time it had not yet been realized that the piston engine was to be completely superseded for aircraft propulsion by the gas turbine, and the development of the aircraft piston engine towards higher power-to-weight ratios was therefore of prime importance to progress in the future of flight. Pye did much to encourage and guide research and development of the two-stroke and the four-stroke piston aero-engines.

During the immediate pre-war years and in the early war years he became closely associated with the development of the new jet propulsion engine, which he did much to encourage. Although his work as Director covered many widely different projects, including Barnes Wallis's famous dam-busting bomb, Pye always retained his special interest in the internal combustion engine, and wanted to write a new up-to-date edition of his book, an undertaking in which he invited me to join him. We also got enthusiastic about writing a similar kind of treatment of the aero-gas turbine, but unfortunately neither venture ever got past the very early stages because Pye soon became so busy in his new post at University College, and I had a new job myself shortly after.

In 1943 Pye entered the last stage of his career when he accepted an invitation to become Provost of University College, London, following the death of Sir Allen Mawer in 1942. Many people feel that during his eight years as Provost he made his greatest contribution, in the extremely difficult and complicated task of restoring the unity of the College, the oldest in the University of London, and setting it on the road to post-war recovery and further development. The invitation appears to have reached Pye at a time when he was feeling tired after several difficult years as Director of Scientific Research, and his decision to accept was no doubt based on a desire to return to academic work and to devote himself to a new task of great constructive interest and value to his country in the coming years. His wife recalls how he told her with enthusiasm of the great reputation of the College and of the opportunity he felt he was being given to make a real contribution to its history.

The war had caused the College to disperse and lose its sense of unity, and it was in danger of disintegrating. Its departments were scattered in six different centres stretching from Leatherhead and Cambridge to Swansea. The buildings were in an appalling condition, the main College building being a gutted ruin, the refectories unusable, and the Union premises and common rooms destroyed. On top of this the new Provost had to face the rather peculiar traditional internal administrative structure, including a Professorial Board with more than usual powers of advising, on which were many eminent men with decided views, some of whom still pressed for secession from the University of London. Whether or not a dictator would have survived under such circumstances is doubtful, but in any case Pye was the very opposite of a dictator, and naturally sought to solve his problems by persuasion. The physical difficulties caused by the blitz, which was still not over, greatly hindered all these tasks, making consultations difficult. For the first eighteen months the Provost and Mrs Pye constantly travelled round visiting colleagues and students, and planning the resumption of work in London and producing an outline of the new building to come. The material obstacles of building restrictions and money, combined with the human problem of finding new staff and, above all, of persuading the varied elements to co-operate, represented a formidable task indeed. Eventually the first buildings were definitely started, and the College was once again set on the road towards unity and the resumption of its place and influence as one of the great London Colleges. He had persuaded many eminent men to return to the College from industry and government, introduced among other things a Chair of Anthropology, and showed always a determination to keep a balance between the arts, sciences and engineering faculties. By the time his retirement was approaching Pye's gentle persuasion and obvious integrity had had their effect.

In all this work he kept in the closest touch with staff at all levels, and showed innumerable small kindnesses. As one of his colleagues has written of this period 'He made us all feel that we were sharing troubles together, that we were a band of improvisers finding companionship and even fun in dealing with each new muddle and complication as it came. Nowhere was this more apparent than in the unforgettable staff parties that he and Mrs Pye gave in such unlikely places as the Eugenics Museum or some Slade studio, parties in which guests in evening dress would pass from a reception in some makeshift place to listen to some lovely music in a classroom, thence via a builders' yard to a severely rationed supper in some horrible hut'.

Thus Pye re-made University College and this is probably the greatest achievement of his career, brought about as it was in such difficult and unusual circumstances which gave play to both his academic knowledge and his many outstanding qualities as a man.

He retired in 1951 but before this he had had a serious illness (mastoid) shortly after taking office, which must have made life more difficult. In 1952 he had the honour of knighthood bestowed on him, and in the same year he became President of the Institution of Mechanical Engineers. His Presidential Address is remembered and often quoted as one of the most thoughtful, particularly on the relation of science and mathematics to engineering. He made the particular point that mathematics is sometimes over-rated, both at universities and in research, in its contribution to engineering design, commenting 'so much of the work of the designer, in mechanical engineering especially, calls in practice for the instinctive skill of the artist rather than for the calculations of the mathematician'. This strong appreciation of the artistic or design side of engineering is consistent not only with his outside interests, but also with his whole scientific life and his contribution to engineering research, in which he was interested in studying the behaviour of complex engineering machines such as the internal combustion engine, and in revealing the essential fundamental elements on which efficient design and performance depends.

Pye was an enthusiastic climber. There are practically no records of his ascents, but he appears to have started serious climbing at the age of twenty-five while at Oxford, when staying at Ogwen. From then on he scarcely missed a season in the Alps, and he constantly visited North Wales. He made his last Alpine expedition in 1947, but continued to walk in Austria or Switzerland until 1956. He also made many visits to the Continent for skiing, at which his sense of balance enabled him to become proficient, if by somewhat unorthodox methods. He usually climbed without guides and often as leader of the party, most of his expeditions being in the Pennines, the Oberland and around Mont Blanc. He also climbed in North Wales, the Lakes and in Skye, where he was with Mallory, Shadbolt and Mrs Mallory in 1918 and led the first ascent of the severe Crack of Doom after climbing the Cioch direct (see notice in *The Alpine Journal* 1960). He was elected to the Alpine Club in 1922, George Mallory being his proposer, and Geoffrey Young his seconder, and became Vice-President in 1956.

In 1926 Pye married Virginia Frances Kennedy, younger daughter of Charles Moore and Eleanor Kennedy. As Virginia Pye his wife was a writer of short stories and also of books for children, and was a younger sister of Margaret Kennedy, novelist and writer. They had one daughter and two sons.

After his year of office in the Institution of Mechanical Engineers we began to see less and less of him in London, and he was visibly a stricken man. The remaining years were spent at his delightful cottage in Surrey, and despite his deteriorating health he continued to find enjoyment in life, particularly in the encouragement of his family in their careers and in their musical interests. There is a delightful picture of family and friends making music together on the lawn of his cottage one summer evening. It is also pleasing to know that he saw a good deal of his old friends the Wimperisses, who lived nearby, at this stage when his health made it difficult to receive those who knew him less well.

The words of Lord Adrian at the memorial service, for which we are grateful, are a most fitting conclusion:

'His physique was not exceptional: he was of medium build, compact and neat in his movements. He climbed deliberately, with no untidy struggles or display of brute force, relying on balance and skill and moving smoothly and seemingly without much effort on the most arduous routes.

'One could see him sometimes as the engineer instinctively judging thrusts and pressures, or as the craftsman confident in his skill and happy in using it. At the summit of a high mountain in the Alps he would rest in the sunlight with little said but in profound contentment, satisfied with the world. He wrote this of his two friends, Mallory and Irvine, lost on the North-East ridge of Everest:

> "Nowhere as among the high snow and ice is the utter insignificance of man's bodily presence so overwhelming, nowhere as among those mighty masses do his desires and aspirations seem, by comparison, so triumphant. Those two black specks, scarcely visible among the vast eccentricities of nature, but moving up slowly, intelligently, into regions of unknown striving, remain for us a symbol of the invincibility of the human spirit."

'David was fastidious and he had the charm of the man of taste and intelligence who can explain his preferences but feels no need to assert their superiority. He was not unconventional, but there was something aristocratic in his nature, an independence and a set of rules and values which were not to be distorted by the fashions of the moment. In Council he was never arrogant or certain that his decision must be right and many of them were not made without stress. It was in fact the high standards he believed in and his honest awareness of difficulties and other points of view which gave him his authority. It was in part because he seemed to be without the ambition to lead that we trusted his leadership. as the director of scientific organization, as the head of University College, or as the first on a climb in the Alps.

'We shall remember David Pye as a charming and civilized man, who gave great service to his country, and made us proud and happy to be among his friends.'

O. A. Saunders

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O. Renner

OTTO RENNER

1883-1960

Otto Renner was a man of retiring disposition who, in a noisy world, made discoveries which quietly transformed our knowledge of his subject. The difficulties that we are bound to encounter in describing his life make it all the more important to find out, while the story is still fresh, how his mind worked, and why he attacked the problems he solved. To do this I shall have to consider his own activities in themselves. But I shall also have to consider how they are related to those of his predecessors and contemporaries whom he supported, displaced, or merely built upon.

I

Otto Renner was born in 1883 at Neu-Ulm. There he was brought up until, at the age of 18, he entered Munich University with a view to studying botany. He became a pupil of Radlkofer and began his career of research with a systematic and anatomic study in certain groups of the mulberry family. After graduating in 1905, on the advice of Goebel, he went to work for a semester under the physiologist Pfeffer at Leipzig. Returning to Munich he became Keeper of the Cryptogamic Herbarium. Here he did not confine himself to his obvious duties of looking after the dried specimens of the lower plants committed to his care. On the contrary he turned to experimental studies and it was with a thesis on water movement in the higher plants that he won his habilitation in 1911. Two years later he was appointed Professor of Plant Physiology and Pharmacognosy.

In 1914 Renner was called away to serve in the army, and for the last 20 months of the war he was working as a bacteriologist in the Ulm Military Hospital. We might have expected that his scientific work would have been broken off. But his published record shows just the reverse. At this period he, so to speak, came into flower. A dozen illuminating papers on physiology and genetics appeared during the war, which was evidently less of a distraction to him than peace-time teaching. And his acknowledgements to his medical friends in Ulm show how and where the work was done.

After the war was over, in 1920, Renner was appointed Professor of Botany at Jena. There he taught and worked for 28 years. But at the end, in the spring of 1945, his laboratory, to which he had devoted so much of his life, was totally destroyed in an air attack. Eight of his colleagues lost their lives and Renner himself had a narrow escape.

Renner's opposition to the Nazi régime was to be expected from his intellectual character as well as from his Catholic upbringing. It brought him no comfort before 1945. After that date it made him the natural representative of the University in dealing with the Occupation Authorities. This, we may judge, was of equally little comfort to him, for three years later he escaped from Jena across the closed frontier to West Germany. On returning to Munich he was appointed Professor of Botany and, although he resigned this post in 1952, he continued to work in the Munich Botany Department until his death on 8 July 1960.

For over 40 years Renner had suffered from continual and incurable insomnia which eventually became worse. He sometimes gave this as a reason for travelling so little abroad. He had visited North Africa as a young man and Italy later in life. And in 1930 he made a long research trip to Indonesia (*das Tropenparadies*, as he called it) for the study of the tropical epiphyte flora whose ecology and physiology, as one could easily learn on a guided tour of his glasshouses, remained an interest with him in later years.

In 1920 Renner married the daughter of a Bavarian army officer. His wife and their married daughter survive him but in 1954 their son was lost as a young student mountaineering in the Alps.

II

Renner's first scientific problem concerned the physiology of water movement, a field in which he was, strangely enough, following the even more notable studies of de Vries. It was in this work that Renner developed the technique for measuring the cohesive capacity of water. His method depended on the secondary explosion of fern sporangia. This simple experiment in biophysics is widely used (although often perhaps misrepresented) in biological teaching today. His interest in problems of permeability and osmotic relations continued with him for many years.

A second physiological problem that attracted Renner's attention was that of phototropic and geotropic movements. Blaauw's theories published between 1914 and 1918, led him to make experiments of his own with gravitation and light stimuli. These were reflected in an article published in 1922.

Renner's work and outlook were from the beginning, and always continued to be, strictly botanical. But it so happens that his deepest interest in life he owed to meeting a zoologist. This was Richard Goldschmidt, a man as different from himself as one can imagine in German academic life. Goldschmidt had learnt about genetics (as he tells us in his Autobiography, *In and out of the ivory tower*) from reading Johannsen's *Elemente der exakten Erblichkeitslehre*. This book, published in 1909, led Goldschmidt to give a course of lectures on genetics at Munich. It also led him to offer a view of his own on the vexed questions of mutation and 'twin hybrids' in *Oenothera*. Goldschmidt's idea was that twin hybrids arose by some kind of 'merogony':

an egg had its nucleus replaced or displaced by a sperm nucleus. Merogony was experimentally fashionable in zoology at the time, and indeed it is well known today in botany under the name of male parthenogenesis or androgenesis. But it had nothing to do with twin hybrids in *Oenothera*. Such was, however, the odd circumstance which led Goldschmidt to obtain stocks of the mutating species of *Oenothera*, presumably from de Vries, and in turn called Renner's attention to the problems of *Oenothera* and mutation, which were to absorb him for the next 30 years.

The scientific history of *Oenothera* was already an old one. Following the introduction of the American evening primroses to Europe, a new species, *Oenothera lamarckiana*, had come to be recognized. De Vries had found an inbreeding population of this plant growing wild on the sand dunes of Hilversum. In the year 1886 he had noticed that some of the plants, one or two per cent of them, were aberrant forms or sports. From breeding these 'mutants' de Vries concluded that mutation, discontinuous hereditary change, was the direct cause of evolution. As such it seemed to him something ultimate; an indivisible unanalyzable event. A similar view was to be held later and longer (and with more justification) of the mutations of *Drosophila*. But, after twenty years, experimental breeding began to reveal an unforeseen complexity in the hereditary behaviour of the unstable evening primroses. When he crossed different species together de Vries often found that the progeny were of two contrasted types; these were what he called twin-hybrids. Reciprocal crosses moreover sometimes gave different types of hybrids. And some species, in America, were found to give no mutations, no twin hybrids, and indeed nothing out of the ordinary at all.

Renner extended de Vries's experiments. He showed that all this behaviour, or nearly all of it, could be understood if he assumed that the species giving twin hybrids were themselves hybrids of a special character. They were hybrid species, hybrid for *complexes* transmitted as units in heredity. They formed gametes of only two kinds but of sharply contrasted kinds. They could therefore give on crossing four contrasted types of progeny. Renner himself would never allow his complexes to be called by names less recondite than *gaudens* and *velans* or *curvans* and *rigens*. But we may allow ourselves to label the complexes by letters and to say that the two species *AB* and *CD* might give progeny of four kinds: *AC*, *AD*, *BC*, *BD*. Usually some of these combinations failed to appear; and different ones according to whether the pollen or the eggs were used in crossing. They were perhaps lethal. So also were the homozygotes *AA*, *BB*, *CC* and *DD*, which failed to appear on self-fertilization,

Renner confirmed this body of hypotheses by doing what his predecessors had failed to do. He examined the structure and germination of the pollen grains, the rates of growth of the pollen tubes, the development of the embryo sac, the behaviour of the embryos and the endosperm, and the germination of the seed. Here he found new and unexpected properties.

The pollen of hybrid species was, as a rule, of two recognizably different types. Often one type would germinate, the other would not. Evidently these types corresponded to the two complexes. Further in some species, the ones which gave different results when used in crossing as pollen or as embryo sacs, the four spores produced by meiosis in the ovule were of two kinds; in half the ovules those at the top of the row, in the other half those at the bottom, developed into embryo sacs. Evidently again these corresponded to the two complexes—which he thereby showed must separate at the first division of meiosis. Evidently also the complex which flourished in the pollen failed in the ovule. Hence the difference between reciprocal crosses. Hence also an economy in reproduction; no homozygotes were formed; none had to be eliminated.

The physiological basis of these interactions naturally interested Renner. The empirical relations of the potential embryo sacs were quite clear. There was a struggle. The top spore was favoured by its position but in half the ovules the bottom spore won by virtue of its nucleus. Occasionally two embryo sacs grew and twin seeds were formed. This sequence of events is what we like to call the *Renner effect.*

What underlay this competition? What was responsible for these differences in behaviour of the same nucleus in different positions and in types of cell? Renner saw that the relations of nucleus and cytoplasm were concerned, 'vielleicht . . . die jetzt im Pollen inaktiven Komplexe gerade als Spermakerne in ein Plasma geraten sind, mit dem sie keine gesunder Pollenkörner zu bilden vermögen' (1921, p. 619). He recognized that there were physiological and also genetic differences between cytoplasms. And he understood that three kinds of factors might be concerned: (i) the action of the nucleus on the cytoplasm in which it lay; (ii) changes in the cytoplasm arising in the course of differentiation; (iii) a genetic character in the cytoplasm itself. But to sort out these several possibilities was too much to expect at that time.

In the light of our present knowledge, of course, it looks much clearer. The third variable can be excluded in this instance, and the first action is seen to be reciprocal. There must be differences in the character of the cytoplasm in different tissues, and there must be a gradient in the kind of cytoplasm of the embryo-sac mother cell. These kinds of cytoplasm react differently with the nuclei carrying the opposite complexes arising by segregation at meiosis. The situation, however, still remains unique as a process in physiology. It also remains unique as a product of evolution.

These discoveries, had they been immediately understood or accepted across the barriers between scientists and also between countries, would have made a great impression. Observing the direct effects of segregation at meiosis confirmed the assumptions of Mendel after fifty years: 'Was Mendel geahnt hat, ist hier mit Augen zu sehen.' They put Mendel's 'elements' in an altogether new setting of physiological activity. The evidence of the reaction of the nucleus and the rest of the cell was novel in biology but very little noticed. The evidence of the gradient in the tissue, if the word gradient had been used, would have extended the ideas of animal into plant embryology. The adaptive and evolutionary character of these properties demonstrated natural selection within what was later to be regarded as a genetic system. But Renner did not enlarge upon these wider implications of his work. He had to concentrate on reaching the end he had in view, the solution of the *Oenothera* problem. To this end all his discoveries were intended to be stepping stones.

Renner's next line of attack was through the principle of crossing over, a process which was being concurrently demonstrated and exploited, and of course disputed, in the study of heredity in *Drosophila.* In hybrids, he maintained, each complex could have small differences chipped off by crossing over with its partner. In many mutants the complex itself was broken into two as though by crossing over. In the next generation one half might then remain intact in some progeny while in others a further batch of mutants revealed, or resulted from, a further disintegration of the complex.

Here Renner was discovering the principle that the character of segregation in natural hybrid organisms always depends on the amount and variety of the differences that are segregating. It depends, that is, on the relations in respect of structure of the chromosomes brought together from opposite parents. Thus when Renner writes (1928, p. 1217): 'welche Faktoren gekoppelt bleiben, und welche getrennt werden können, hängt von dem Partner ab, mit dem mann einen gegebenen Komplex verbindet,' he is saying something which probably applies not just to hybrid species of *Oenothera*, but to all outbreeding organisms from *Amoeba* to man.

Renner thought of these changes in terms of the assumption of crossing over. But he rejected a final analysis in terms of genes (or 'lethal factors') just as he did in terms of mutations. It was in this respect that he seemed furthest behind the times but proved to be furthest in advance of them. He saw and recognized the integral character of his complexes and the integral actions of their genotypes. He refused to express them in terms of hypothetical genes in those cases where he had—and we are likely to have—no analytical evidence. On the other hand wherever the analytical evidence was available, from his physiological and embryological observations, he introduced it into his genetical theory.

III

Soon after Renner had laid out his general scheme it became clear that his rules of heredity were going to be matched by equally unusual rules of chromosome behaviour at meiosis in *Oenothera.* These were disclosed by the work of Cleland at Baltimore. The precise relationship, the full interpretation, of course only slowly became clear. Indeed we may say it is still in process of becoming clear today.

In the first place, Cleland found that Renner's hybrid species all had multiple rings of chromosomes at meiosis. Usually all 14 were arranged in a ring with seven alternate members passing to each pole. The alternate chromosomes carried the alternative complexes. When de Vries's mutations arose the ring was found to have broken into two halves. It then gave a ring of six and four pairs. Or it had recombined to give seven pairs which had once more a free Mendelian recombination. Thus Renner's assumption that the complexes had broken into halves or broken into pieces was paralleled by the changes in the behaviour of the chromosomes.

How this system had come into being, what evolutionary processes in the breeding system, the chromosome system, and the geography of *Oenothera*, underlay it, the answers to these questions reached beyond Renner's concern. They spread their influence and are still spreading it into apparently remote fields, the theories of polymorphism, of sex chromosomes, of population genetics, of the origin of species. Indeed they are re-occupying for us the territory which de Vries at the end of the last century had thought to have occupied with his mutation theory.

His immediate problem Renner had solved. And in solving it he had laid, incidentally rather than deliberately, the foundations of certain new methods and disciplines. On the one hand, there was now an integral genetics which was prepared to study physiology, embryology, and evolution and to relate them all without attempting impossible or invalid analysis in terms of ultimate units or genes. And on the other hand, there was an experimental embryology of plants which was partly, but only partly, paralleled on the animal side. For the nuclear differentiation proved to be matched by, and adapted to, tissue gradients as well as tissue differences. This is a situation which only the extraordinary character of the hybrid species combined with the elaborate alternation of plant generations could ever be expected to reveal.

IV

The implications of Renner's discoveries were beyond the capacities of one man, one school, or even the geneticists of one country, to follow to the end. Many of these, particularly so far as they concerned the chromosomes, their evolutionary and physiological properties, Renner did not attempt to follow except as a routine matter. These were not fields in which he felt so much at home. He turned his attention to two groups of problems which are worth describing because in them he was able to exploit his own extraordinary gift, the gift for seeing and for interpreting a plant in terms of its whole character as well as of its separable characters.

From the beginning Renner had established that the general heredity of the hybrid *Oenothera* species was a great perversion of the ordinary rules of Mendelism or Morganism. But Renner's scheme, as we have seen, allowed him in general terms to reconcile the systems of the evening primrose with those of the pea and the fly. It was towards the end of this work that he came upon a second perversion of Mendel's rules quite unrelated—or so it seemed—to complex hybridity. This was *gene conversion.*

It had been clear to Mendel himself that the non-mixing, the atomic purity, the clean separation, of the alternative elements, or genes as we call them, was the indispensable and revolutionary part of his hypothesis, the crux of his system. Yet, in 1937, Renner found that in certain heterozygotes a particular mutant gene *cruciata* and its normal allele were capable of interaction. The heterozygous plant became homozygous, *Aa* became *AA*, in the course of its vegetative development. Usually the change was from the recessive to the dominant normal allele; but it could be the reverse, *Aa* becoming *aa*. The direction of change apparently was influenced by the genotype as a whole.

Gene conversion, or something like it, has since been inferred in yeast and other fungi, and in *Drosophila*. Its meaning, and above all the question as to whether it has a fundamental meaning, as it formally appears to have, eluded Renner's enquiries for twenty years and still eludes ours.

V

The second and greater problem derived from *Oenothera* to which Renner devoted himself was that of the inheritance of plastids. This problem has both mechanical and physiological sides and on both sides he was able to reach conclusions. His success was due to the skill with which his previous experiments enabled him to handle the nuclear side of heredity. Hybrids and mutants, in plants generally, show that the plastid has a genetic character which can be stable in its reactions. In consequence it can be permanently transmitted in heredity. The same is true of the nucleus and even of the cytoplasm. We may accordingly say that the green pigment of leaves is produced by the harmonious interaction of nucleus, plastids, and cytoplasm. And a change in any one of them may lead to failure to produce a stable chlorophyll or enough of it.

This was the most plausible scheme in 1930 or thereabouts. In his hybrid species of *Oenothera*, Renner had the means of testing it. He had plants which produced two widely divergent types of gamete in respect of the nucleus; and hence in crosses between species four types of nucleus and four recognizably different types of plant. At the same time different species which could be crossed had divergent types of plastid. Moreover the pollen and the eggs both carried plastids. They therefore gave mixed cells in hybrid zygotes. Now, in *Oenothera* hybrids, as in any other hybrids, the mixed chromosomes sort themselves out suddenly and exactly at meiosis. But the plastids sort themselves out gradually and irregularly during development. Hence an array of combinations of chromosomes and plastids can be produced by breeding. In Renner's hands these proved to be conclusive. They showed that plastids which were green with their habitual nucleus became white with certain foreign nuclei. But they could be kept alive in chimaeras, that is in the white layers or sectors of cells produced by sorting out. They could then be handed back (by back crossing) to their old nuclei when they always turned green again. This *tour de force* was also the crucial experiment. It demonstrated the genetic autonomy and genetic permanence of determinants or genes in the plastids.

Renner's work was the basis of studies which are still developing on the genetic relations of the plastids, their capacities for self-propagation and competition, and of course their individual structure. Although the plastids are no doubt genetically more complex and physiologically more versatile, their structural development might be expected to tell us something about their smaller neighbours in the cytoplasm. Renner's latest experiments on differentiation in ivy began to open up this new field for the study of genetic particles.

Renner's studies have thus provided methods and models for attempts to understand genetic particles of all kinds, those which are responsible for infection, differentiation and immunity, as well as those responsible for heredity itself. And the consequences are likely to prove as important for animals as for plants even though their source is so remote as to be forgotten.

VI

To assess a man's scientific work we need to compare it with what his predecessors achieved. We may well think, with satisfaction in these days, that no experimenter has ever handled plants in so many ways with such mastery as Renner did. But how does he compare with those before him who dealt with the same plants, the same methods and the same ideas as himself? First we think of the great figure of de Vries whom Renner himself commemorated in heroic terms twenty-five years ago.

Renner and de Vries began life in very different positions; for Renner was the son of a schoolmaster in a small town in Bavaria, de Vries, the son of the Prime Minister of his country. Once in the university however, their enquiries began to drive them along parallel paths. Both started research as physiologists. Both used plant cells to solve physical problems. Both became professors at 30. Both thereupon changed their course and took up the study of genetics, the genetics of *Oenothera*.

Yet in what they did they were deeply contrasted. De Vries had written massive volumes propounding his solutions to the Darwinian problems of genetic particles and mutations. These works which were based on prolonged but somewhat ineffective experiments made his name famous. Renner, on the other hand, by skilful design and apt observation solved de Vries's problems. But he published his results in technical language and in professional periodicals read only by serious students.

How technical Renner's writing could be, we may see from his first remarkable paper written at Ulm in September 1917. It begins with a sentence which condenses within its twelve lines the essence of what he knew about the breeding of *Oenothera*. No great discovery could have been more obscurely revealed. It was a case of open concealment. Renner had the justification for making those brilliant generalizations which de Vries had wished to make. But Renner would not make them. Perhaps he did not wish to be too easily understood. Certainly he dreaded that he should be misunderstood.

This, however, is not the whole story. Renner had a genuine fear of abstraction and generalization. He liked to think of his complexes, which were a good abstraction and a necessary inference, as 'empirische Realitäten'. He was therefore alarmed—alarmed though not perhaps seriously shocked or ultimately disappointed—to find others resting far from empirical statements on his foundations.

We might suppose that the contrast between Renner and de Vries was due to a change of habit or fashion. But if we compare Renner with Mendel we learn otherwise. Renner's discoveries displaced the ideas of de Vries; they developed the principles of Mendel. Renner's complexes and Mendel's elements are both notions to which their authors were forced by their own experiments. Both are consequently notions which have unfolded their meaning to us only slowly with the passage of time. And the similarity in the methods and ideas of the two experimenters truly reflects, I believe, the similarity in their minds and characters.

VII

Apart from his own research Renner was devoted to the editing of journals. From 1933 to 1943 he was concerned with *Flora*, from 1947 to 1956, with *Planta*, and from 1949 to 1955, with *Fortschritte der Botanik*. And since 1947 he was on the editorial board of the *Zeitschrift für Naturforschung*.

Renner was Dr.h.c. of Jena, Erlangen and Freiburg. He was Ordinary Member of the Leopoldina and Bavarian Academies and Foreign Member of the Washington and Vienna Academies as well as of the Royal Society to which he was elected in 1955. In 1952 he received the Order 'Pour le Mérite' of Science and Arts, and in 1959 the Darwin medal, of the Leopoldina.

I wish to thank two of Renner's pupils, his successor, Professor Leo Brauner, and Dr Franz Schötz, who have taken great trouble in providing me with information which I have used in preparing this account.

C. D. Darlington

Note.

For *de Vries:* see *Obit. Not. R.S.* **4**, 371-3, Dec. 1935; and Renner, 1936 (below).

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LOTTE MEITNER-GRAF
LONDON

E. Schrödinger

ERWIN SCHRÖDINGER

1887-1961

Erwin Schrödinger was born on 12 August 1887 in Vienna. His father was a Viennese and came from a very cultured family. The ancestors of his mother came from England which explains why Schrödinger spoke and wrote a practically flawless English. The relationship between father and son was excellent. To the growing boy the father was friend and teacher and he never tired of answering the inquisitive questions which the bright and interested boy produced in plenty.

His elementary education was private and was carried out at home. At the age of 11 he entered the Akademische Gymnasium, a first-class grammar school which emphasized classical education, Latin and Greek. In spite of his main interest in mathematics and physics he loved the old languages as well as classical German and foreign poetry, and he was an addict of the famous Burgtheater in Vienna.

He entered the University of Vienna in 1906. His great and beloved teacher there was F. Hasenöhrl who had just succeeded to the chair of Boltzmann and who considered it his privilege and duty to continue the work of this great theoretical physicist. Hasenöhrl was a very gifted physicist indeed and probably he would have been capable of great achievements had he not been killed in 1916 serving at the Italian front. He exercised a profound and lasting influence on the student Schrödinger. When much later Schrödinger received the Nobel Prize he said: 'had Hasenöhrl not been killed so young he would now be here in my place.' Schrödinger himself served during the war as an artillery officer in Italy.

From 1910 onwards his first publications began to appear. They were concerned with statistical problems, dielectrics, magnetism, and so on. Later on he also worked on X-ray diffraction in lattices, following the great discovery of von Laue in 1912.

In 1920 he married a girl from Salzburg, Annemaria, and with her he went to Jena to accept a junior post under Max Wien. Only four months later he was called as junior professor ('ausserordentlicher Professor') to Stuttgart and soon afterwards three professorships were offered to him (Kiel, Vienna, Breslau). He accepted Breslau, but again only for one term. Already another offer was on the way—by the University of Zürich. In the field of theoretical physics Zürich could boast of great names; A. Einstein and M. v. Laue were his predecessors in this chair (Einstein was the first Professor of Theoretical Physics in Zürich). He was destined to continue this great tradition. Six years later he was to become their colleague in Berlin.

The six years in Zürich are the most important period in Schrödinger's scientific life, so that it is convenient at this point to interrupt the account of his life to pay attention to his work.

As soon as the war had ended Schrödinger resumed his scientific work. The general theory of relativity was produced in 1916 and there are several interesting papers by Schrödinger on this subject. Statistical mechanics and X-ray diffraction figure again among his publications, as well as a new subject which one might perhaps call a hobby: the theory of colours and vision. It was not until 1921 that Schrödinger became actively interested in the fundamental problems of quantum theory. At that time Bohr's theory of quantization of periodic orbits was current but no one had any illusions that this was the final solution of the problem. Schrödinger also published a few papers on these lines. There is, for example, an important paper on the Doppler effect which he could explain by making use of Einstein's formula for the momentum of a light quantum $h\nu/c$. In 1925 his interest turned again towards statistical mechanics, and this proved to be of no small importance. In a paper published by the Prussian Academy he showed that a certain connexion exists between two different statistical methods which were much discussed at the time: Boltzmann's treatment of the ideal gas had to be amended by Planck's famous division by $N!$—'because the molecules of a gas are indistinguishable'. On the other hand, Einstein had proposed a very different statistical treatment of a gas following a method used by Bose for the statistical treatment of light quanta in black-body radiation. This method (since then called 'Bose-Einstein statistics') proved to be one of only two really consistent treatments of truly identical particles. Schrödinger showed that Planck's *ad hoc* division by $N!$, is identical with Bose-Einstein statistics at the high-temperature limit. Einstein's paper, however, had a sequel. In a second paper Einstein investigated the fluctuations in a gas treated by Bose-Einstein statistics and found that these consisted of two parts: (i) a part which could truly be attributed to fluctuations of particles and (ii) a part which had the same form as the fluctuations of a wave field due to interference. Thereupon, Einstein remarked that it was not impossible that particles had something like a 'wave nature' just as electro-magnetic waves had something like a particle nature (light quanta). And he drew attention to a publication by de Broglie (published, I believe, with Einstein's encouragement), where an attempt had been made to treat particles as waves.

Upon this remark by Einstein, de Broglie's paper was widely studied in Germany. I was a student at the time preparing for my Ph.D. in Munich under Sommerfeld, and de Broglie's paper was discussed there too. Everyone had objections (they were not very difficult to find) and no one took the idea seriously. I believe this applied to most theoretical physicists—except Schrödinger. He studied de Broglie's work and set out at once to establish a proper wave theory of electrons. The key to his success was that he imposed a restriction on himself. Whereas de Broglie intended a relativistic quantum theory, for which the time was not yet ripe, Schrödinger confined himself to Newtonian mechanics translated into quantum theory. In an outburst of genius and productivity he developed 'wave mechanics' within a few months and published it in his four classic communications to the *Annalen der Physik*. In these four papers wave mechanics was established in its final form with the inclusion of enough applications to be entirely convincing.

Independently, a different development took place in Göttingen and Cambridge. Starting from more abstract considerations, Heisenberg was able to set up a quantum mechanics of the harmonic oscillator, from which Born and Jordan on the one hand, and Dirac on the other, were able to establish the general laws of quantum mechanics in matrix form or in terms of non-commuting quantities. There was to be no quarrel between the two lines of development for Schrödinger was able to show very soon that the two were entirely equivalent.

Schrödinger's work was soon rewarded. In 1927 Planck retired in Berlin and Schrödinger was to be his successor. I joined Schrödinger's department in Zürich with a postgraduate fellowship during the last half year of his stay in Zürich. F. London was also there. It was a very pleasant summer and, of course, Schrödinger was in excellent spirits. Each Sunday a small excursion was arranged by Mrs Schrödinger which invariably ended in some nice country inn with a glass of wine (or maybe two). He liked the country and the hills. It was not without some misgivings that he accepted the Berlin offer. The big city of the north, he thought, did not suit his Austrian temperament so well, although he was really very happy there.

Berlin as a cultural and scientific centre was unique at the time. For theoretical physics Planck, Einstein, v. Laue, and now Schrödinger were there. It was second to no other place in the world. The following six years were a fruitful and happy period.

With Schrödinger's work quantum mechanics was born but it was still somewhat in its childhood. The wave equation was correct and final (as far as the non-relativistic theory was concerned) but it showed up only one, though very important, aspect, the wave aspect. And the meaning of the 'wave function' was not entirely clear. Schrödinger at first interpreted it quite simply as a new field, like the Maxwell field and nothing else. Then came the 'statistical interpretation' of Born together with the transformation theory of Dirac and others, the uncertainty relation and so forth. All this was mainly developed in Göttingen, Copenhagen and Cambridge and it completed the final structure of quantum mechanics. The Berlin theoretical physicists, Einstein, Planck, v. Laue and Schrödinger were all very reluctant to accept it. 'I can't imagine that an electron hops about like a flea', Schrödinger would say. Einstein tried to construct 'Gedanken-experimente' (experiments carried out in the imagination only) to find some contradiction in the statistical theory and these were invariably disproved, one by one, in a similar manner by Bohr. By and by, Schrödinger would agree that the psi-function did have a statistical meaning, but he never accepted a theory as final in which a certain lack of determinism prevailed. He was reared in the classical scientific tradition, which was entirely deterministic, and he could not think in any other terms. His disappointment went deep. There is an element of tragedy in the situation which undoubtedly influenced his further scientific life.

In Berlin Schrödinger continued to publish important papers, of which the 'Zitterbewegung', a corollary to Dirac's relativistic theory of the electron, may be mentioned.

Then came the ominous year 1933. Schrödinger did not conceal his distaste of the Nazi regime and when Professor Lindemann (later Lord Cherwell) from Oxford came to Germany to invite dismissed German scholars to Britain, Schrödinger accepted his offer of a post at Oxford. The Nobel Prize, which he shared with Dirac, was already on the way and the news came shortly after his arrival in England. In 1936 he was offered professorships in Graz (Austria) and Edinburgh. His longing for the home country must have prevailed against a better political judgement—he went to Graz. What was to be expected, happened. In 1938, Hitler invaded Austria, and as Schrödinger had committed himself in 1933 as an anti-Nazi, he had to leave. He fled to Rome. As a member of the Pontifical Academy he probably thought to find shelter there.

At that time Mr Eamon De Valera, himself a mathematician, and still interested in science, then Prime Minister of Eire, intended to establish an 'Institute for Advanced Studies' in Dublin, modelled after the Princeton Institute. (There are now many such Institutes in the world.) It had to be relatively cheap, so at first only two 'schools' which worked with paper and pencil only, were founded, the School of Celtic Studies, and the School of Theoretical Physics. With the many refugee scholars about and with the generosity of the Irish Government towards this purely scientific and non-practical Institution (such generosity was not so usual in the pre-war days) it was not difficult to find professors for the school of theoretical physics. Mr De Valera approached Schrödinger in Rome and Schrödinger accepted the invitation to be the first Professor for Theoretical Physics in the new Institute.

I joined the Institute in 1941 when it was formally opened with a symposium on meson theory. The Institute was (and is) housed in one of the lovely Georgian buildings in Old Dublin, very suitable and comfortable for quiet work unburdened by mass lecturing and the modern curse of the scientists —administration. The spirit in the new Institute was excellent. De Valera was a frequent visitor, and the Irish colleagues and senior students were full of enthusiasm. Schrödinger worked on a variety of subjects. There are papers on meson algebra, thermodynamics and on the non-linear electrodynamics of Born and Infeld. But his chief love was general relativity. He made several attempts at a 'unified field theory' combining the gravitational and electromagnetic fields. Essentially, he had left the main line followed by most of the younger theoretical physicists, which was concerned with going deeper into the problems of relativistic quantum field theory and theory of elementary particles. The chief reason, I believe, was the fact that he did not think that ordinary quantum mechanics was finished yet. He was still reluctant to accept the statistical version of the theory as final, and therefore it was no use for him to go beyond it.

Life in Dublin was peaceful, even during the war years and afterwards. Like almost everybody else Schrödinger would cycle the three miles from his home to the Institute (only a few of the wealthier people could afford horse-drawn traps), and he would go for long excursions on his bicycle. His study in his home was very simple. At times it gave more the impression of an artist's studio than of a scientist's study. He liked modelling, and often one could see a small statue that revealed his great artistic flair. His interest in the classics never ceased, he even wrote a book on Greek science. His schooldays obviously had had a profound influence on his whole life. His interests were very wide indeed. The new discoveries in the field of genetics fascinated him so much that he wrote a book *What is life?* in which he discussed the relationship between living matter and physics. Later, in Vienna and a few years before his death, he wrote about the relationship of science and religion.

Throughout his life Schrödinger was a lone worker. Very rarely he accepted research students. There were a few, though, in Dublin. There is hardly any publication with joint authorship of which Schrödinger was a partner. Nothing could have been more foreign to him than the now fashionable 'team work'. All his work bore the imprint of personality and individualism.

Schrödinger remained scientifically productive until about 1955 although his health was gradually giving way. He began to suffer from frequent attacks of bronchitis and asthma and, of course, the damp Irish climate was not particularly suitable for him in this condition. Once more his beloved homeland called him. In 1957 he was offered a chair, created personally for him, in Vienna. His health as well as his love for the place of his birth prompted him to accept. But his health did not greatly improve. After a prolonged illness he died in Vienna on 4 January 1961, in the city of his birth. It was his wish to be buried in a small Tirolian village, Alpach, which he loved. It was a simple and quiet burial.

W. HEITLER

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Hugh Scott

HUGH SCOTT

1885-1960

Hugh Scott was born on 16 September 1885, at Lee, London, S.E., the son of William Edward Scott (1855-1920), of the London Stock Exchange and grandson of George Robert Scott (1817-1888), also of the London Stock Exchange. His mother was Edith Truscott, daughter of James Chapman Amos Truscott, M.Inst.C.E., and of Jane Wyatt Truscott. On his father's side he was a direct descendant of the family of Scott of Scot's-Hall in East Kent, a family which has been traced back for some centuries. Hugh Scott's own family circumstances were at first modest but his father prospered and in 1896 moved to London. The parents were cultivated, fond of art, music and literature and lovers of travel but had no particular interest in any branch of natural history. Scott's development as a naturalist cannot in fact be attributed to any family or ancestral influence. Like many boys, he began to collect butterflies—at about the age of seven—but while this kind of thing is usually a passing phase, like stamp collecting, it became, with Hugh Scott, his life-interest. However, his special passion, the love of natural history expeditions, was probably fostered by the frequent vacation journeys on which he accompanied his parents, from about the age of ten.

Throughout his life Hugh Scott was handicapped by rather vague but no less real ill-health. This caused his removal, first from a preparatory school in Blackheath and then from Westminster School to which he had won a classical entrance Exhibition, but left after little more than a term. Nevertheless, under a private tutor, he passed the London Matriculation 1st class, in 1903 and the Cambridge 'Little-Go', the same summer. He entered Trinity College, Cambridge, in 1903, passed the first M.B. examination in 1904, won an Exhibition at Trinity in March 1906 and a First Class in the Natural Sciences Tripos, Part I, in June 1906.

At his first interview with his Director of Studies, Scott asserted his fixed determination to follow the career of a field naturalist. His choice of entomology seems to have been due to the influence of Dr David Sharp, whose vast knowledge of insects is evident in the classical volumes he contributed to the *Cambridge Natural History*. In his fourth year at Cambridge, Scott began entomological research under Sharp, thus forfeiting the opportunity to take Part II of the Tripos, which he would certainly have passed with distinction. This, I feel, was a sacrifice of academic distinction to the passion for investigation, laudable, certainly, but in all probability a strategic error; because a 'First' in the Second Part of the Tripos often leads to a College Fellowship, whereas Scott's early research work, though, as always, conscientious and accurate, could hardly be considered as anything but an exploration of well-trodden paths in biological science. The systematic treatment of certain Hawaiian Coleoptera, as carried out by Scott, was a valuable contribution to the important *Fauna Hawaiiensis* and is linked with his later work; but such studies, along classical lines, could not be expected to produce spectacular results.

In June 1908 Scott went to the Seychelles as a member of the Percy Sladen Trust Expedition, led by the late Professor J. Stanley Gardiner. After the return of the leader to England, he remained camping in the endemic mountain-forests of the larger islands until March 1909. The results occupied his attention and that of 49 other specialists for a period of 25 years. The 'General conclusions regarding the insect fauna of the Seychelles and adjacent islands' were completed by Scott in 1932 and appeared in the *Transactions of the Linnean Society* in 1933.

In March 1909 Scott was appointed Curator in Zoology at Cambridge in succession to Dr David Sharp but shortly afterwards the title of the position was changed to Curator in Entomology—an alteration, as Scott may have felt, suggesting a certain restriction of function and closing a door that hitherto seemed to be open. However, Scott held the position for 19 years, during which time the entomological section of the Museum of Zoology expanded to more than three times the space of the old Insect Room, the collection of British insects was greatly improved, series of teaching exhibits were instituted and many research students were guided in their work; lectures on the taxonomy and natural history of insects were also given to advanced students.

In 1910 and 1912 Scott made short journeys to the West Indies, visiting Trinidad and Jamaica, which gave him an opportunity to compare the tropical islands of the New World with the Seychelles. In 1917 and 1918 he did war work as a civilian entomologist in the Hygiene Department, Royal Army Medical College, and gave lectures on insect vectors of disease to army officers. During the period from August 1926 to February 1927 he made an expedition to the highlands of central Abyssinia with Professor J. Omer-Cooper. Trekking on horse or mule and living under canvas for many weeks, he covered some 500 miles in several separate journeys. During this expedition some 40 000 insects were collected, with hundreds of other invertebrates and many plants. Some 40 or 50 reports on this material have been published. One cannot help but marvel at the tenacity of purpose evidenced by these long and arduous journeys, by a man whose health was generally regarded as indifferent.

About 1928, a crisis occurred in the life of Scott. The Department of Zoology in Cambridge, in which entomology had been handled by Scott as Curator and by a Lecturer, decided to create a readership. It was natural for Scott, after his long years of service, to feel that he was a suitable candidate for this post; but it eventually became apparent to him that he was not so regarded by the authorities. Even to a man as modest and apparently unambitious as Scott, this was a very heavy blow. Against the advice of many friends, he resigned his position in Cambridge to become Entomologist in the Government Department of Agriculture in Iraq. Arriving in Baghdad at the beginning of the hot weather, he was struck down by amoebic dysentery which was followed by a general breakdown in health. After four months he was invalided home to England but before leaving managed to make an inspection tour of Kurdistan. For almost two years he was without a position but during this time he visited Basutoland in order to recuperate, camping at the foot of South Africa's highest mountain and riding up it. In September 1930 he was appointed as an Assistant Keeper in the Department of Entomology of the British Museum (Natural History) from which he retired in July 1948 to have more freedom for his personal research.

However, during his period of work in the Museum and after his retirement, he continued his explorations. In 1937 and 1938, with Mr E. V. Britton, he visited the mountainous parts of the Western Division of the Aden Protectorate and the closed Kingdom of the Yemen. In 1948-1949 he travelled through the Gughé Highlands in Southern Ethiopia, collecting mainly insects and plants and published a general report on the journey in 1952. In 1952-1953, he went via Eritrea to Northern Ethiopia, where he explored the high Simien district, trekking with a caravan of mules and horses for 46 days. We may recall that this expedition was made in Scott's 68th year. After his return he continued, in his retirement at Henley-on-Thames, to work up the results of his expeditions.

In any attempt to assess the interest and importance of the work of Hugh Scott, it must be remembered that he was primarily the curator of an entomological museum and a biological explorer. These activities are complementary and if conscientiously pursued, bring certain great advantages. To be a good curator, a man must have a wide knowledge of his materials. Other things being equal, this makes him a good potential explorer. But a good curator wishes not merely to maintain but also to enrich his museum and who can do this better than a good explorer? But these same functions imply certain real disadvantages. Neither the curator nor the explorer can easily concentrate on anything in particular. The intensive investigation of special groups, required for fundamental advances in taxonomy and the application of such methods as are used in genetical or physiological studies, is sometimes a practical impossibility. Hugh Scott's knowledge of insects was very extensive. Anything of special interest attracted his attention and he published a large number of observations which could and may have stimulated detailed and useful studies. So far as I am aware he did not carry out comprehensive revisions of any large groups but his taxonomic studies ranged over a wide field and all of these show the same scientific competence and painstaking accuracy, for Scott was not only a good anatomist but also an excellent artist. It must be added that he was a recognized authority on the curious Diptera of the family Nycteribiidae, parasitic on bats, and made important contributions on the relationship between certain plants and insects, notably, on the fauna of the Bromelias.

These contributions were, however, either incidental or by-products of Scott's main life-long effort which was in the field of biogeography. For over twenty years, with remarkable pertinacity, he pursued, with the help of some fifty specialists of thirteen different nationalities, the classification and study of the 50000 insects collected in certain archipelagos and single islands of the Western Indian Ocean: the Seychelles and adjacent islands.

The difficulties of such an enterprise should not be underestimated. No one man can pretend and Scott did not attempt to deal adequately with this enormous mass of material. But he was obliged, nevertheless, to sort it into groups with which particular specialists could deal. Suitable specialists had then to be found and persuaded to undertake work which sometimes perhaps could not easily be fitted into their research programmes, and by unrelenting but tactful prodding had to be kept going and induced to prepare their results for publication. From the latter part of 1910 onwards, wrote Scott, 'the unofficial editorial work needed to prepare the various authors' writings for press has absorbed more and more of my time, and has retarded completion of my personal contributions'.

The enumeration of the species of animals or plants from a particular locality or region of which no accurate census has previously been made is certainly in itself a work of scientific merit. But, if it is only a catalogue, it is of limited interest in spite of its practical usefulness. To discover what they mean, the data must be carefully analyzed and considered in relation to the ecological background and with regard to the problems of general biology.

Scott spent eight months collecting in the Seychelles and he had at his disposal material collected in briefer visits by other naturalists. That these collections constituted an adequate sample of the insects of the area, sufficient to permit of general conclusions, might be doubted. However, the present writer has found that collections of the Tachinid Diptera in Trinidad, spread indeed over a longer period but certainly less intensive, ceased after some years to produce much that was new, though Trinidad is close to the South American mainland and therefore easily accessible to invasion by the continental fauna. The total area of the Seychelles is hardly more than one-tenth that of Trinidad and they are extremely remote oceanic islands, situated more than a thousand miles from the nearest continents. It seems, therefore, intrinsically probable that Scott's material was an adequate sample of their insect fauna, sufficient, at all events, to justify all the conclusions based on it. A fact that supports this belief is the consistent difference found between the coralline islands, as compared with the mountainous granitic islands, with their rich endemic forests. Aldabra, one of these coralline islands, had been visited by previous expeditions and in 1908-1909 J. C. F. Fryer spent four and a half months there, devoting all the time he could to the collection of insects. Nevertheless, and in spite of the fact that Aldabra is larger and ecologically more suitable for insects than the other coralline islands, its insect fauna, like those of the others, was very meagre in comparison with the faunas of the granitic islands.

The chief ecological interest of the Seychelles is that they are really remote oceanic islands, which were uninhabited until their colonization by the French during the eighteenth century. As a result of the colonization a great deal of the original endemic forest disappeared and some foreign plants and animals were introduced. Nevertheless, the Seychelles are too remote to be a tourist area and were, at the time of Scott's investigations, difficult of access. They were therefore very suitable for the study of the oceanic type.

In order to bring out the salient characteristics of the fauna, Scott carried out a searching and detailed comparison with that of another oceanic archipelago, the Hawaiian islands. These are also tropical and remote from continents; but markedly different in many ways from the Seychelles. They are volcanic, not plutonic, and have been subjected to extensive glaciation, of which there are no signs in the Seychelles. They are 43 times as large, with far higher mountains and the remaining mountain forest is far more extensive. They have, of course, been inhabited for a much longer period and the importation of foreign animals and plants has been much greater. Nevertheless, Scott calculated that the original endemic fauna of the Seychelles was at least as rich as the Hawaiian fauna in relation to the size of the islands. Certain very ancient orders of flying insects—the Plecoptera and the Mecoptera—are inexplicably absent from both the Seychelles and Hawaii. The Ephemeroptera and the Trichoptera, absent in Hawaii, which is ecologically suitable for them, exist in the Seychelles. As they are weak fliers, they would, presumably, find it difficult to cross wide expanses of ocean. The Seychellean species are endemic. These facts, Scott considered, are among the many indications that the Seychelles must in the remote past have had a land connexion with a continent; but this does not explain why the most recent of the four Orders just considered—the Trichoptera—exists in the Seychelles, while the older Plecoptera and Mecoptera do not. Though some ancient Orders are absent from the Seychelles and Hawaii, practically all those regarded as recent are present. From this Scott concluded that both of these faunas originated after all the existing Orders of living insects were differentiated, but with the impartial objectivity that characterized his thinking, he offered the alternative possibility that an evolutionary development from ancient and presumably very different ancestral forms followed the same course as in other parts of the world. Considered in relation to the usual modern view of organic evolution, this was a refreshingly unorthodox suggestion.

Pressing his comparison a little further, Scott observed that although the number of insect genera in the Seychellean fauna is greater than in Hawaii, the ratio of endemic genera to all genera is greater in Hawaii. In fact, said Scott, the fauna of the Seychelles has the aspect of a remnant of an ancient continental fauna; that of Hawaii, the aspect of a development from a few ancestral immigrants, which, in the course of the ages, gave rise to large genera or groups of genera, composed of endemic species. Within the Seychellean archipelago, Scott showed, there are numerous cases of closely allied, but distinct species, inhabiting ecologically similar but geographically isolated areas, an example being two species of the Coleopterous genus *Bourdonnaisia*, each confined to the extreme mountain peaks on the two islands of Mahé and Silhouette.

Another point of great interest brought out by Scott—though noted by some of his predecessors in the study of oceanic islands—is the small size and dull colouring of the vast majority of the endemic species of the Seychelles, compensated, so to speak, 'by a wonderful diversity of form and structure and by the profusion of individuals of very many species'. Some biologists would have been tempted to explain the small size and dull colour as a protective adaptation produced by natural selection, but Scott produced a more plausible and useful suggestion; that the peculiarities in question, if they are really characteristic of the insects of oceanic islands, are simply the result of certain special climatic conditions and of no particular adaptive significance. However, after offering this suggestion, he went on to point out that brightly coloured insects are everywhere uncommon and can hardly be expected to be abundant where the fauna is very small—which is to say that perhaps we have here a mere pseudo-problem.

One of the classical examples of unverifiable evolutionary speculation, to which students of biology are still often subjected, is that the reduction of wings is particularly common in the insects of oceanic islands and that this is the result of natural selection, because the insects able to fly and addicted to flying are carried out to sea by the winds sweeping the islands; an alternative explanation being that the insects prudently cease to fly and that their wings become atrophied through disuse. Scott's comments on these ideas are, in the first place, that the percentage of flightless forms in the Seychelles is not exceptionally high and in the second place that the variety of situations in which flightlessness occurs makes it difficult to believe that it is an advantage and has 'survival value'. The mountains of the Seychelles, at any rate, he notes, 'are densely forest-clad to the summit, with almost no bare exposed places from which insects, unwisely taking to the wing, could be blown out to sea'. Again, the reduction of eyes, though it was found in some Seychellean Coleoptera, had, in Scott's view, no definite relation to environmental conditions. Eyeless species are commonly regarded as inhabitants of caves, where their condition is supposed by some to be in some way advantageous. Scott points out that there are no caves in the Seychelles.

Passing from these questions of a general character, Scott proceeded to an analysis of the data concerning particular groups. This minute and painstaking discussion cannot be summarized here. It led to the general conclusion, already mentioned, that the endemic fauna of the Seychelles is a continental remnant, the direct land connexion having been severed a very long time ago. But whether the connexion was an unbroken land bridge or never more than a chain of islands, Scott felt unable to say. Nevertheless, in his essay on the insect fauna of the Seychelles, he seems to have extracted from the data all the conclusions for which he had reasonable evidence, without wasting time on far-reaching, unverifiable speculations; and his work has been well described by one of his collaborators as 'a model of clarity'.

It is surprising that with Scott's vast knowledge of natural history and the experience gained in his faunistic work, he did not round out his career by producing a general treatise on biogeography, for which he was exceptionally well equipped. Perhaps he would have regarded this as arm-chair science and found it distasteful. He liked the direct contact with nature and maintained to the end his active interest in biological exploration. His later expeditions to Abyssinia and the Yemen did not provide him with material sufficient for such extensive and thorough studies as he made for the Seychelles. They did, however, as has been mentioned, produce valuable results. On the British Museum expedition to South-Western Arabia in 1937-38, Scott wrote a number of important works, among them a book entitled *In the high Yemen*, comprising some 260 pages with 113 photographs, 4 maps and 8 figures. This covered the history of the country from ancient times as well as the manners and customs of the people. It was highly commended by reviewers with a first-hand knowledge of the region and went into a second edition. Later, Scott contributed to a *Geographical Handbook* (B.R.527): 'Western Arabia and the Red Sea', published for official use by the Naval Intelligence Division of the Admiralty. Another evidence of Scott's broad interests was his paper on early clay tobacco-pipes found in Cambridge, published in 1917 by the Cambridge Antiquarian Society.

In 1913 Hugh Scott married Beatrice Emily Streatfield, a descendant, like himself of an old Kent family. He was much interested in genealogy and took some trouble to learn the history of his wife's family. He found that it can be traced back to Robert Streatfield of Chiddingstone Manor, who lived in the 16th century. What he apparently failed to notice—judging from his Personal Record—was that by his marriage to Beatrice Streatfield he brought into the Scott family one of those authentic descents which are the delight of the expert, as distinct from the amateur-genealogists: the descent through the wife of Henry Streatfield (1757-1829) from the Northumbrian family of the Ogles of Ogle who held, early in the 12th century, lands their descendants still occupied after an interval of more than eight hundred years. Beatrice Scott died in 1947 but a son and a daughter survive.

Personally, Hugh Scott was a kindly, modest and quiet man always willing, even at the expense of his own studies, to put his knowledge at the disposal of anyone genuinely interested in natural history. His extensive biological learning and wide experience made him a valued member of the Committees of many organizations and learned societies. In 1926 he served as Vice-President of the (Royal) Entomological Society of London. But his retiring disposition, due in part perhaps to his indifferent health, but also to a real aversion to anything like self-advertisement, prevented him for a long time from securing the recognition less able colleagues easily obtained.

The value of his research was indeed recognized by the award of the Cambridge D.Sc. in 1919, but it was not until 22 years later that he was elected a Fellow of the Royal Society. In 1954 he became an Honorary Fellow of the Royal Entomological Society of London.

Scott's parents, he tells us, were members of the Church of England. He was brought up in what might be termed a liberal but genuinely Christian atmosphere. His biological studies did not undermine his Christian convictions, which became progressively stronger throughout his life. In so far as his scientific studies and physical limitations permitted, he was active in the work of his Church, often visiting the Trinity College Mission in a very poor district of South London, serving on Parochial Church Councils and maintaining an interest in the Christian missionary work at home and overseas. Old age brought no decline in his scientific interests and, until his deeply regretted death on 1 November 1960, he was still pursuing his studies and preparing his results for publication.

W. R. Thompson

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1928. (With Dr Malcolm Cameron.) Staphylinid Coleoptera from Rodriguez Island. *Ann. & Mag. Nat. Hist.* (10), **1**, 270-283.
1929. An entomological tour in Kurdistan. *Ent. Mon. Mag.* **65**, 69-82, pl. 3-5.

1929. On some cases of maternal care displayed by cockroaches, and their significance. *Ent. Mon. Mag.* **65,** 218-222.

1930. A naturalist in Basutoland. *Discovery,* **11,** 52-55.

1930. A new East African species of *Urodon* (Coleoptera), bred from *Gladiolus,* with notes on the Biology of the genus. *Ent. Mon. Mag.* **66,** 104-109.

1931. Note on the use of the hind legs as weapons by the males of *Deporaüs betulae. Ent. Mon. Mag.* **67,** 241-243.

1932. (With Dr A. V. Schulthess.) Some more South African Masaridae, with notes on the mouth-parts of the genera *Quartinia* and *Quartiniella. Ann. & Mag. Nat. Hist.* (10), **10,** 525-536.

1932. Entomological research in the Marquesas Islands, *Nature, Lond.* **130,** 797.

1933. The systematic position of the family hemipeplidae (Coleoptera), and descriptions of new species. *Ann. & Mag. Nat. Hist.* (10), **12,** 595-611.

1950. The stormy threshold of the Antarctic. (Review-article based on Dr R. Jeannel's 'Au seuil de l'Antarctique.') *American Scientist,* **38,** no. 4, 568-79 and folding map.

B. Short Notes and Small Papers

(These vary in length from one paragraph to several pages; almost all of them deal with natural history of insects, though the first was an ornithological query.)

1903. 'Two broken eggs': under this (not quite accurate) title a query by Scott appeared in *Nature Notes: the Selbourne Society's Magazine,* **14,** 177. (Scott was just under 18 years old.) 'In late June of that year I had found the eggs in two yellow buntings' nests and a song-thrush's nest with the upper sides broken and the eggshells quite empty but not crushed. In the October number of the same magazine (p. 217) W. Percival Westall M.B.O.U. suggested that the culprit was a jay. I had found the nests at Greys Green near Henley-on-Thames, and I recalled the occurrence 49 years later, in late June 1952, when I saw a number of small birds mob a jay and drive it out of my garden at Henley; and I afterwards found, on the ground near where the jay was mobbed, a song-thrush's egg with one side broken and the contents sucked out of the shell in just the same way.'

1907. Note on the life-history of (the fly) *Mycetaulus bipunctatus. Ent. Mon. Mag.* **43,** 160.

1908. The pupal and adult stages of a fly new to Britain (parasitic on leaf-hopper bugs), *Pipunculus melanostolus. Ent. Mon. Mag.* **44,** 9-11.

1914. (The beetles) *Hoplia philanthus* near Cambridge and *Homaloplia ruricola* in Oxfordshire. *Ent. Mon. Mag.* **50,** 221-222.

1915. Lepidoptera, Hymenoptera, Neuroptera and Siphonaptera collected in the Outer Hebrides in 1914. *Scottish Naturalist,* pp. 252-254.

1915. Butterflies of the Chilterns. *Entomologist,* pp. 208-212.

1915. Swarm of (the weevil) *Barypithes pellucidus* in Cambridgeshire and notes on its food-plants. *Ent. Mon. Mag.* **51,** 218-219.

1915. (The wood-wasps) *Xiphydria prolongata* in Cambridgeshire and X. *camelius* in the New Forest. *Ent. Mon. Mag.* **51,** 266-267.

1916. Note on the swarming of Chloropid flies, Psocidae, etc., in houses. *Ent. Mon. Mag.* **52,** 18-21. Further note on same, pp. 43, 44.

1916. Another Staphylinid (beetle) parasitic on a Dipteron. *Ent. Mon. Mag.* **51,** 206.

1916. Breeding of (the Sawfly) *Cimbex lutea. Ent. Mon. Mag.* **51,** 262.

1917. A note on *Vespa sylvestris. Ent. Mon. Mag.* **53,** 17.

1919. Swarming of the Chalcidid *Pteromalus deplanatus.* Bees in buildings. *Ent. Mon. Mag.* **55,** 13-16.

1920. Insects damaging lead and other metalwork. *Ent. Mon. Mag.* **56,** 10-12.

1920. Butterflies, beetles and queen wasps on the wing in (mid-) winter. *Ent. Mon. Mag.* **51,** 64, 65.

1920. Note on the life-history of (the froghopper) *Triecphora vulnerata*. *Ent. Mon. Mag.* **51,** 136-137.

1920. Notes on (i) the parasitic staphylinid (beetle) *Aleochara algarum* Fauvel and its hosts, the phycodromid flies; (ii) a case of supposed parasitism in the genus *Homalota*. *Ent. Mon. Mag.* **51,** 148-157.

1921. The ptinid beetle, *Trigonogenius globulum* Solier. breeding in Argol. *Bull. Ent. Res.* **12,** 133, 134.

1921. Notes on the oviposition and habits of the Iris Sawfly, *Rhadinoceraea micans* Klug. *Ent. Mon. Mag.* **57,** 229-232.

1922. Note on some hymenopterous parasites and other enemies of *Tortrix viridana* (defoliator of oaks); with further records of Chalcididae swarming in buildings. *Ent. Mon. Mag.* **58,** 56-61.

1923. Longevity of a cerambycid larva. *Ent. Mon. Mag.* **59,** 90.

1923. *Sirex gigas*; early appearance, and other habits. *Ent. Mon. Mag.* **59,** 113, 114.

1923. (Notes on) (i) 'A genuine British specimen of (the hover-fly) *Volucella zonaria*' (ii) 'the habits of (the fly) *Milesia crabroniformis*'. *Ent. Mon. Mag.* **59,** 260.

1923. List of Orthoptera and Neuroptera collected in the Eastern Pyreness, with oecological notes. *Ent. Record.* **35,** 122-126; 146-149.

1924. Coleoptera, Trichoptera, Diptera, etc., at light. *Ent. Mon. Mag.* **60,** 154.

1925. Records of Coleoptera at light and in houses. *Ent. Mon. Mag.* **61,** 113-115.

1925. Note on the nesting-habits and prey of two British species of (solitary wasps) Crabronidae. *Ent. Mon. Mag.* **61,** 156-160.

1925. (The parasitic fly) *Pipunculus horridus* in Britain. *Ent. Mon. Mag.* **61,** 162.

1925. Note on the occurrence of (the powder-post beetle) *Lyctus villosus* in the Hawaiian Islands. *Proc. Hawaii. Ent. Soc.* **6,** 213-214.

1926. Note on the swarming of gnats or midges round lofty towers. *Ent. Mon. Mag.* **62,** 18-19.

1926. (The wood-wasp) *Sirex gigas* (The horse-botfly), *Gastrophilus equi* and other insects on bare mountain-tops. *Ent. Mon. Mag.* **62,** 19, 20.

1926. Coleoptera in the sea. *Ent. Mon. Mag.* **62,** 115, 116.

1926. A rare variety of (the Small Copper) *Chrysophanus phlaeas* in North Wales. *Ent. Mon. Mag.* **62,** 116.

1926. Coleoptera, sawflies and Tipulidae drowned in the sea. *Ent. Mon. Mag.* **62,** 165-168.

1926. Ants collecting seeds of *Chelidonium majus* (the Greater Celandine). *Ent. Mon. Mag.* **62,** 241.

1927. Notes on some foreign Coleoptera imported into Britain, and their biology. *Ent. Mon. Mag.* **63,** 181, 182.

1928. Occurrence of the bat-bug, *Loxaspis mirandus*, in Portuguese East Africa, and note on the association of some Coleoptera with bats. *Ent. Mon. Mag.* **64,** 108-109.

1928. Note on swarms of typhlocybid bugs on *Phlomis* and other plants. *Ent. Mon. Mag.* **64,** 116.

1929. Note on the life-history of the fig-tree moth, *Ocnerogyia amanda* Staud. (Lymantriidae). *Bull. Ent. Res.* **20,** 39-40.

1929. Introductory note to 'Embioptera from Baghdad' by P. Esben-Petersen. *Ent. Mon. Mag.* **65,** 7.

1929. *Brachypterolus pulicarius* damaging *Linaria purpurea*, *Ent. Mon. Mag.* **65,** 159, 259-260.

1929. Biological note on the South African beetle *Urodon lilii* Fåhraeus, and Accidental transport of South African insects overseas by human agency. *Proc. Ent. Soc.* **11,** pp. 99-101.

1929. Blind instinct or conscious action? (Review-article.) *Ent. Mon. Mag.* **65,** 161-162.

1930. The fauna of the Hoggar. *Ent. Mon. Mag.* **66,** 5-7.

1930. Note on a mass-flight of *Pyrameis cardui* near Baghdad and on the capture of an example at sea. *Ent. Mon. Mag.* **66,** 31-32.

1930. A mortal combat between a spider and a wasp. *Ent. Mon. Mag.* **66,** 215.

1930. Insects at light. *Ent. Mon. Mag.* **66,** 232.

1931-1932. Section J (Hymenoptera) of Appendix to Bertram Thomas's *Arabia Felix,* 1932 (pp. 361-362); (previously published in *Geogr. J.* **78,** No. 3).

1931. The origin of the class insecta and other matters. (Review-article.) *Ent. Mon. Mag.* **67,** 115-116.

1932-1933. Cells, believed to be made by mason-bees, from a torrent bed in Central Arabia. *Proc. Ent. Soc.* **7,** 46-48 (second note, *Proc. Ent. Soc.* **8,** 9-10).

1933. Random notes from North Oxfordshire, 1933. *Ent. Mon. Mag.* **69,** 279.

1934. *Plutella porrectella* L. infesting sweet rocket in Oxfordshire. *Ent. Mon. Mag.* **70,** 137-138.

1934. Notes on *Cionus scrophulariae* L. infesting a South African plant, *Phygelius capensis* E. Mey. *Ent. Mon. Mag.* **73,** 29-34: and second note, p. 68 (1937).

1937. An observation on *Bombus terrestris* L. *Proc. R. Ent. Soc. Lond.,* **A12,** 51.

1941. Immigrant butterflies in 1941. *Ent. Mon. Mag.* **77,** 254.

1942. *Pieris brassicae* feeding on honesty. *Ent. Mon. Mag.* **78,** 192 (second note, Dec. 1942, p. 282).

1943. Hibernation of *Chrysopa carnea* Stephens in unusual numbers in a house. *Ent. Mon. Mag.* **79,** 63.

1943. Swarming of *Psectrosciara tenuicauda* Duda (Diptera, Scatopsidae). *Ent. Mon. Mag.* **79,** 156-157.

1944. Notes on the season of 1943. *Ent. Mon. Mag.* **80,** 1-4.

1944. *Apemea* (*Xylophasia*) *monoglypha* Hufn. (Lep., Agrotidae) as prey of bats. *Ent. Mon. Mag.* **80,** 155.

1944. *Dorytomus rufulus* Bedel (Curculionidae) bred from a hybrid sallow. *Ent Mon. Mag.* **80,** 206-207.

1944. Black ants (*Acanthomyops niger* L.) collecting rosemary seeds and dead Coleoptera. *Ent. Mon. Mag.* **80,** 207-208.

1944. *Cantharis sp.* ?*rustica* Fall. (Col., Cantharidae) hovering in numbers round trees. *Ent. Mon. Mag.* **80,** 211.

1945. Immigrant and other Lepidoptera in the Henley district, 1944. *Ent. Mon. Mag.* **81,** 62.

1945. Chloropid flies swarming round a fragrant clematis. *Ent. Mon. Mag.* **81,** 97.

1945. Rainfall in relation to scarcity or abundance of wasps. *Ent. Mon. Mag.* **81,** 97, 98 (second note on this subject on p. 277 of this volume).

1945. A queen humble-bee in December, 1944. *Ent. Mon. Mag.* **81,** 140.

1945. Larvae of the orange-tip (*Euchloë cardamines* L.) feeding on sweet rocket. *Ent. Mon. Mag.* **81,** 276.

1947. *Colias croceus* Geoffr., *C. hyale* L. and other immigrant Rhopalocera in Britain in 1946. *Ent. Mon. Mag.* **83,** 13.

1947. Abundance of *Plusia gamma* L. (Lep., Noctuidae) in 1946. *Ent. Mon. Mag.* **83,** 55.

1948. A congregation of common blues (*Lycaena icarus*). *Ent. Mon. Mag.* **82,** 303 (second note, same phenomenon in same place. *Ent. Mon. Mag.* **84,** 167).

1948. Sexual attraction(?) between butterflies of different species. *Ent. Mon. Mag.* **84,** 124.

1948. *Colias croceus* in Galloway. *Ent. Mon. Mag.* **84,** 260.

1950. *Paederus fuscipes* Curtis (Col., Staphylinidae) and other migrants insects attracted to a ship's lights in the Red Sea. *Ent. Mon. Mag.* **86,** 217-218.

1951. Swarming of *Megaselia meconicera* Speiser (Diptera, Phoridae) in houses. *Ent. Mon. Mag.* **87,** 269-270.

1952. Oviposition in *Bombylius* (Diptera, Bombyliidae). *Ent. Mon. Mag.* **88,** 216.

1953. Diptera found indoors, winter season 1951-1952. *Ent. Mon. Mag.* **89,** 87-88. (And a postscript note, p. 102.)

1953. Millipedes entering houses. *Ent. Mon. Mag.* **89,** 216.

1953. Discrimination of colours by *Bombylius* (Diptera, Bombyliidae). *Ent. Mon. Mag.* **89,** 259-260.

1953. A large flight of water-beetles, and their attraction to light. *Ent. Mon. Mag.* **89,** 260-261.

C. Reviews, Abstracts, Biographical Notices, etc.

It is unnecessary to list fully a large number of Reviews, Abstracts and Biographical Notices; the following may be mentioned:

1916. Review of the volume on Tineid Moths in the *Biologia Centrali-Americana. Nature, Lond.* **96,** 533.

Also review of:

1927. Miss Cheesman's *Islands near the Sun. Ent. Mon. Mag.* **63,** 161-162.
1929. W. M. Wheeler's *The Social Insects: their Origin and Evolution. Ent. Mon. Mag.* **65,** 210-213.
1932. G. J. Arrow's *Fauna of British India, Coprinae. Ann. & Mag. Nat. Hist.* (10), **10,** 133-134. (An essay-review, entitled 'The Rhopalocera of Abyssinia' (based on *The Rhopalocera of Abyssinia: a faunistic study* by Professor G. D. Hale Carpenter), *Sci. Progr.* **31,** 155-157.

Antiquarian paper.

1917. Early clay tobacco-pipes found . . . (in) Cambridge (with bibliography). *Cambr. Antiq. Soc. Communications,* **20,** 147-168, pls. 23, 24.

Biographical Notices.

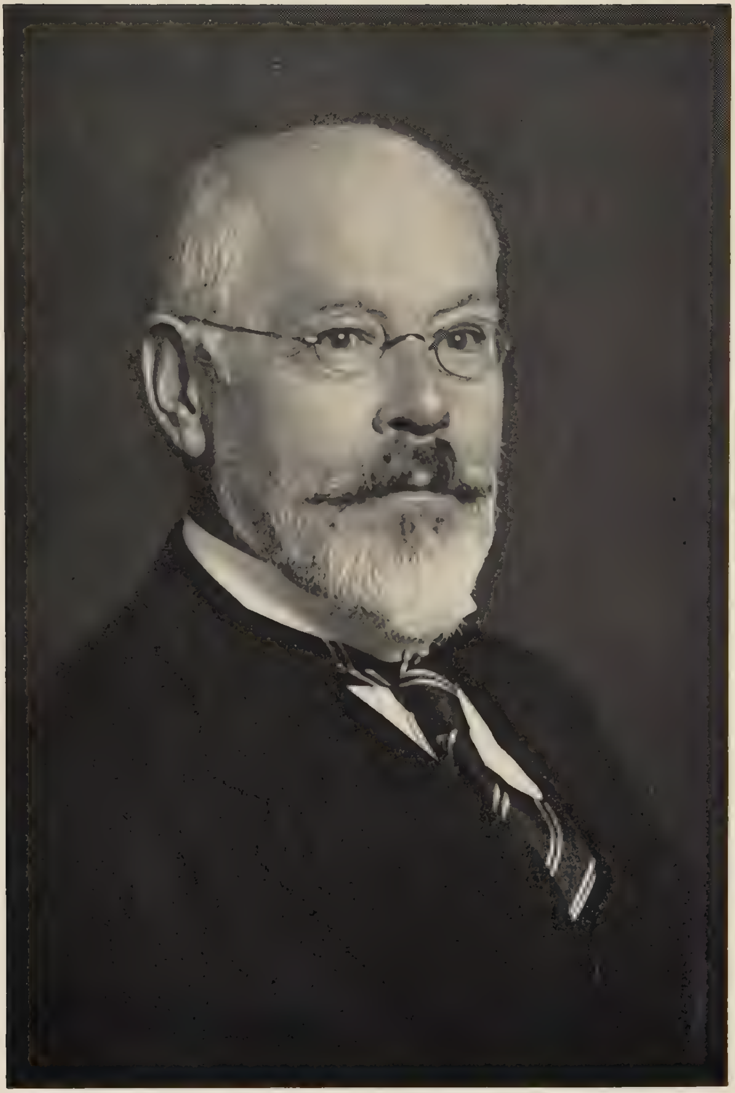
Among others, these include:

1922. David Sharp, *Nature, Lond.* **110,** 521.
1923. Francis J. H. Jenkinson (University Librarian, Cambridge). *Nature, Lond.* 6.x.1923, 516; and *Ent. Mon. Mag.* **59,** pp. 261, 262.
1931. Frederick Muir. *The Times,* 22 May.
1934. Lt.-Col. Charles George Nurse (soldier, traveller, linguist and student of natural history, especially entomology). *Ent. Mon. Mag.* **70,** 20, 21.
1947. Rev. Alfred Thornley (1855-1947, pioneer of nature-study in schools, etc.). *Ent. Mon. Mag.* **83,** 109, 110.

D. Reports of the Curator in Entomology (in the University Museum of Zoology, Cambridge)

Scott wrote a report each year from 1909 to 1928 (both years inclusive). In the first two reports he was still designated 'Curator in Zoology', the title he held by his predecessor, Dr David Sharp, F.R.S. (to whom he paid a tribute in his first Report); after that Scott was called 'Curator in Entomology'.

Scott's first four reports (dealing with the years 1908 to 1911) formed part respectively of the 43rd, 44th, 45th and 46th *Annual Report of the Cambridge Museums and Lecture Rooms Syndicate.* That Syndicate ceased to exist in 1912, and his subsequent reports appeared annually in the Report of the 'General Board of Studies on the progress and condition of certain University Departments', published in the *Cambridge University Reporter.* (Also in April 1922, Scott circulated an important 'Memorandum on the Insect Room' which, however, was stencilled, not printed.)



L. J. Spencer

LEONARD JAMES SPENCER

1870-1959

Leonard James Spencer, formerly Keeper of Minerals at the British Museum (Natural History) from 1927 to 1935, died in London on 14 April 1959, at the advanced age of 88.

He was fourth in a line of distinguished Keepers of Minerals in the British Museum, his predecessors being Story-Maskelyne, appointed first Keeper in 1857, Sir Lazarus Fletcher and G. T. Prior. All three were Fellows of the Royal Society to which Spencer was elected in 1925. He was born at Worcester on 7 July 1870, the eldest of eight children of James Spencer, schoolmaster, and Elizabeth Bonser, and was educated at Bradford Technical College (1883-1886) where his father was for many years headmaster of the day-school department.

With a Royal Exhibition Spencer proceeded to the Royal College of Science for Ireland in Dublin (1886-1889) where he gained first-class honours in chemistry in 1889.

Later he entered Cambridge University as a scholar of Sidney Sussex College. Here he studied geology, mineralogy and chemistry, gaining a first class in both parts of the Natural Sciences Tripos and was awarded the Harkness Scholarship for Geology (1893). Funds were very short for Spencer at College and he lived quietly and kept very much to himself. Already at Cambridge he was abstracting papers for H.M. Patent Office (Abridgments of specifications of Philosophical Instruments for the period 1855-1892), and so embarked, perhaps from necessity, on a work that was later to become a labour of love.

It was fortunate for mineralogy that just at the end of his Cambridge course there happened to be one of the infrequent vacancies in the scientific staff of the Mineral Department of the British Museum, and for this post Spencer was the successful candidate.

The latter part of this year (1893) he spent at Munich studying under Groth, Weinschenk and Muthmann, returning in late December to take up his post of assistant on 1 January 1894. So began an official connexion with the British Museum which lasted through forty years, but intimate association with the Mineral Department lasted for another twenty-four years to the time of his death.

Spencer's achievements fall into three main fields, his work as curator, as an original investigator, and as editor and bibliographer. He was a born collector and has placed on record his early interest as a boy 'in making and collecting things, gardening and keeping animals'. At the age of seven his father presented him with a geological hammer, and fossils were early collected by him on the Yorkshire coast. By 1892 his collection of minerals, rocks and fossils contained some 3000 labelled and catalogued specimens; these he offered to, and were gratefully accepted by, Bradford Technical College for the use of their students.

On his appointment to the Mineral Department of the British Museum he entered with enthusiasm on his curatorial duties. He first worked with Miers on the mineral collection and on the departure of Miers to a Chair at Oxford the whole of the work on the collection came into Spencer's hands, though later some of this devolved on Herbert Smith who was appointed in 1897. Spencer's main achievement, apart from his scientific research, was the establishment of the system of registration, labelling and cataloguing, and it is mainly through his efforts that this collection has come to rank as the best documented and indexed in the world.

His output of original research was considerable and his publications, more than 150 in all, range over a wide field of descriptive mineralogy. Many of his early papers stemmed from the curatorial work he did on the crystallographic catalogue.

He described as new the minerals miersite, tarbuttite, parahopeite, chloroxiphite, diaboleite, schultenite, aramayoite and bismutotantalite, besides supplying much new data on incompletely described species. Among such contributions were his paper on plagionite ($Pb_5Sb_8S_{17}$), heteromorphite ($Pb_7Sb_8S_{19}$) and semseyite ($Pb_9Sb_8S_{21}$) as members of a natural group of minerals forming as he supposed a morphotropic series. These formulae have been generally accepted, and it is of interest to note that the mineral fülöppite ($Pb_3Sb_8S_{15}$) discovered much later (1929) has been included in the morphotropic relationship.

In the same year appeared also the well-known paper of Spencer & Prior on the identity of binnite with tennantite, and the chemical composition of fahlerz.

By the end of the first decade of his work at the Museum he had published 27 papers. The value of these contributions to mineralogy were appropriately recognized by the Geological Society of London by the award of the Wollaston Fund to him in 1902.

When Spencer succeeded Prior as Keeper in 1927 he carried on his predecessor's work on the great collection of meteorites, and his interest was greatly stimulated by his visit to South-West Africa in 1929 where he studied the meteorites of Gibeon and Hoba. In succeeding years the records show 19 published papers dealing with meteorites or meteorite craters.

His meteorite studies were model investigations of their kind and he had the co-operation of his colleagues, particularly M. N. Hey, in dealing with their chemical investigation.

The discovery of silica-glass fragments at meteorite craters (Henbury and Wabar) led him eventually to take up the problem of tektites and their origin.

These he believed were the products of the impact of large meteorites on the earth. It was about this time (1934) on the eve of his retirement that his zeal led him to accept an invitation to join an expedition to study the remarkable masses of silica-glass found by P. A. Clayton in 1932. He failed however to find the hoped-for evidence of meteorite craters and returned with the problem of the origin of the glass still unsolved.

Spencer's lively interest in meteorites endured to the end, his last publication being a communication on the subject to *Nature* of March 1958.

Spencer wrote two books on minerals and gemstones, which have had a wide circulation: *The world's minerals* (1911) and *A key to precious stones* (1936).

Earlier he had translated from the German Max Bauer's large quarto work *Edelsteinkunde* (1904) and Reinhard Brauns's *Das Mineralreich* (1908-1912).

The third field of Spencer's activity was as editor, bibliographer and abstracter, and in this domain he gave immense service to his science.

His work as abstracter began soon after his appointment to the British Museum. From the first he contributed abstracts to the *Mineralogical Magazine* and as early as 1895 he was preparing abstracts in the field of mineral chemistry for the *Journal of the Chemical Society*; later he contributed to the *Annual Reports* of the same Society.

He took over the editorship of the *Mineralogical Magazine* from Miers at the close of 1900 and continued to edit it until the end of 1955.

He prepared the indexes of the *Magazine* covering in all twenty volumes and these appeared in two issues (1895, 1926). He supplied triennially lists of new mineral names (21 lists in all) and obituary notices of mineralogists (eight series) and three supplementary lists of British minerals.

In 1920 the Mineralogical Society commenced publication of *Mineralogical Abstracts* with Spencer as editor. These appeared as a sequel to the *International Catalogue of Scientific Literature* which terminated in 1914. During this period as editor (1920-1955) twelve volumes were issued and he lived to see *Mineralogical Abstracts* appear in a new format as a joint publication of the British and American Mineralogical Societies aiming at a still wider international coverage of mineralogy.

The Mineralogical Society celebrated Spencer's fifty years as editor of the *Magazine* by the issue of a Jubilee number in 1950 to which colleagues at home and abroad contributed papers, and on the occasion of the anniversary meeting in November 1950 a dinner was given in his honour.

Spencer was a meticulous and persistent editor as many who had papers pass through his hands well knew. He had a remarkable capacity for attention to every detail of a paper and had a sufficiently good knowledge of many languages and a good enough memory to enable him to detect every flaw in the spelling or printing of titles. He has indeed left a record that he possessed dictionaries in forty-one languages. No one who worked with him could fail to appreciate the depth of his knowledge, his limitless industry and power of concentration and beneath a somewhat brusque manner, a saving sense of humour and a truly kind heart.

In 1899 Spencer married Edith Mary, daughter of Islip J. Close of Mortimer, Berkshire. Mrs Spencer was a gifted woman able in the early years of their married life to help with the translation of Bauer's *Edelsteinkunde*. She died in 1954. They had a son and two daughters, and it was with pride that Spencer saw his son creating a flourishing engineering business and his two daughters rising to the front ranks of their profession each in her own special line, one in ballet and the other in orchestral music.

Spencer made three long trips overseas, primarily for the purpose of collecting specimens and when possible of visiting museums and private collections.

One of these trips was to Canada in 1924 for the British Association Toronto Meeting when he visited many mining districts and museums in Quebec and Ontario and the Eastern United States collecting a large amount of material for the Museum.

The second to South Africa in 1929 for the British Association meeting in Johannesburg and Pretoria, and the International Geological Congress, visiting the Kimberley, Vaal River and Premier diamond mines, Lusaka zinc mine, Broken Hill mines and the copper belt in Northern Rhodesia. These visits were extended to the diamond fields and mines at Tsumeb and Grootfontein in South-West Africa.

Finally the special expedition of the Survey of Egypt in 1934 into the Sand Sea of the Libyan desert to investigate the occurrence of silica-glass to which reference has already been made.

The last decade of Spencer's service at the British Museum brought him honours and promotion richly deserved.

He was elected a Fellow of the Royal Society in 1925 and Correspondent (later Honorary Fellow) of the Mineralogical Society of America (1926), and Honorary Member of the German Mineralogical Society (1927). He was made C.B.E. in 1934.

Later came the award of the Murchison Medal of the Geological Society of London (1937) and the Roebling Medal of the Mineralogical Society of America in 1940.

He served as President of the Mineralogical Society (Great Britain) (1936-1939) and became its Foreign Secretary (1949). He continued an active member of the Society to the end, attending regularly meetings of the Council and of the Society itself. He served also as a member of his successor's team of abstracters for *Mineralogical Abstracts*.

He had been a member of the Mineralogical Society for 65 years and for long an honoured and familiar figure on the front bench at its meetings.

As we look back on his achievements, the unique service he gave to his science, we realize the magnitude of the loss it has sustained. In the annals of the Society he served so well his passing marks indeed the end of an epoch.

In the compilation of this notice much assistance has been provided by the appreciative accounts of Spencer's work and life, published by Dr W. Campbell Smith (*Mineral. Mag.* **29,** 256-270 (1950) and **32,** 181-184 (1959)). These sources are now gratefully acknowledged.

C. E. Tilley

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1895. (With G. T. Prior.) Augelite. *Mineral. Mag.* **11,** 16-23.

1895. Enargite. *Mineral. Mag.* **11,** 69-79.

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1897. (With G. T. Prior.) The identity of andorite, sundtite and webnerite. *Mineral. Mag.* **11,** 286-301.

1898. Miersite, a cubic modification of silver iodide. *Nature, Lond.* **57,** 574.

1899. Plagionite, heteromorphite and semseyite as members of a natural group of minerals. *Mineral. Mag.* **12,** 55-68.

1899. (With G. T. Prior.) The identity of binnite with tennantite, and the chemical composition of fahlerz. *Mineral. Mag.* **12,** 184-213.

1901. Marshite, miersite and iodyrite from Broken Hill, New South Wales. *Mineral. Mag.* **13,** 38-53.

1901. Crystallized stannite from Bolivia. *Mineral. Mag.* **13,** 54-65.

1902. (With G. T. Prior.) The cerargyrite group (holohedral-cubic silver haloids). *Mineral. Mag.* **13,** 174-185.

1907. Notes on some Bolivian minerals (jamesonite, andorite, cassiterite, tourmaline, etc.). *Mineral. Mag.* **14,** 308-347.

1908. On hopeite and other zinc phosphates and associated minerals from the Broken Hill Mines, North-Western Rhodesia. *Mineral. Mag.* **15,** 1-38.

1908. (With H. J. Johnston-Lavis.) On chlormanganokalite, a new Vesuvian mineral. *Mineral. Mag.* **15,** 54-61.

1919. Mineralogical characters of turite (= turgite) and some other iron ores from Nova Scotia. *Mineral. Mag.* **18,** 339-348.

1919. (With J. E. Stead.) The ternary alloys of tin-antimony-arsenic. *J. Inst. Metals,* **22,** 127-148.

1920. Fibrolite (= sillimanite) as a gem-stone from Burma and Ceylon. *Mineral. Mag.* **19,** 107-112.

1920. Identity of Trechmann's 'β tin' with stannous sulphide. *Mineral. Mag.* **19,** 113-123.

1921. Curvature in crystals. *Mineral. Mag.* **19,** 263-274.

1923. New lead-copper minerals from the Mendip Hills (Somerset). *Mineral. Mag.* **20,** 69-92.

1924. Allopalladium from British Guiana. *Mineral. Mag.* **20,** 217-219.

1925. International agreement in mineralogical and crystallographical nomenclature. *Mineral. Mag.* **20,** 353-363.

1926. Sperrylite crystals from the Transvaal. *Mineral. Mag.* **21,** 94-97.

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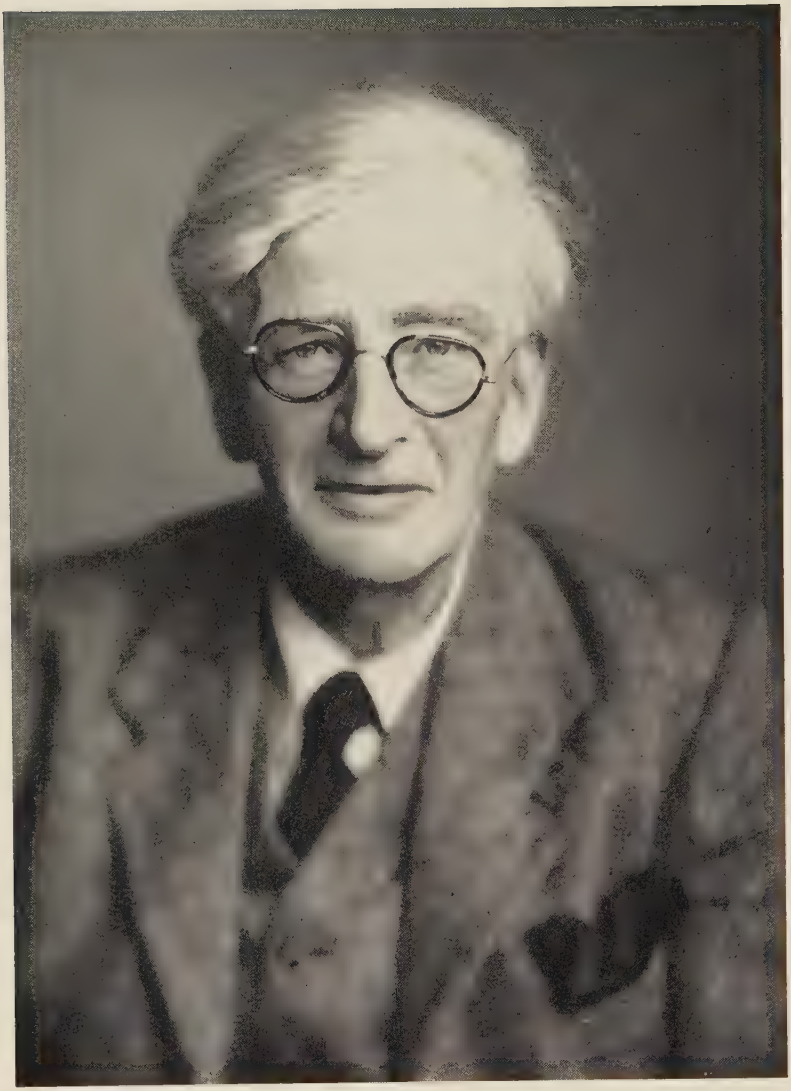

REGINALD GEORGE STAPLEDON

1882-1960

Reginald George Stapledon, born 22 September 1882, at Lakenham, Northam, near Bideford, came of a long line of North Devon landowners and farmers; a family tradition linked them with Walter de Stapeldon (1261-1326), Bishop of Exeter and founder of Exeter College, Oxford. His grandfather, James Stapledon, of Bideford, had, however, like many other men of Devon, broken away from the land and became a master mariner; his father, William (1829-1902), did likewise, sailing his own barque from Appledore and included among other enterprises much gun running to South America. The log survives of an adventurous voyage in 1852 to Australia where most of the crew deserted to take part in the gold rush. He was capable and forceful, vehement in condemning the bad conditions under which shipowners expected their captains and crews to sail, very observant and with an almost poetic appreciation of nature—traits which reappeared markedly in young George. Shortly after the Suez Canal was opened in 1869 he set up a shipping agency in Suez and Port Said; it prospered and brought him wealth, the friendship of de Lesseps, and the confidence of the Canal community. All this necessitated long absences from home, and when finally he returned he was smitten by a stroke which kept him bed-ridden for some years before he died in 1902. Perhaps for these reasons he seems to have little direct influence on George's development.

The outstanding influence of George's childhood was his mother, Mary, daughter of William Clibbett, the last builder of wooden ships in Appledore; a man of pronounced literary tastes and strong character who obstinately refused to abandon wood for iron or to turn off his workpeople—and he died penniless, but owing nothing. Mary was very beautiful; she had had little formal education and was largely self taught; she was an enthusiastic admirer of Darwin, Huxley and other advanced thinkers of the time and became an agnostic: free thinkers they were then called. George was devoted to her and she to him; she had ample leisure, a staff to relieve her of household duties, and some social isolation resulting from the fact that she was her husband's second wife, the first having been her sister, and such marriages were not much approved in those days. Like her husband she was a great lover of nature and took George botanizing over Braunton Burrows, then floristically rich; she took him also for long drives in the byeways of their attractive district, and gave him a small garden where he grew flowers and fruit for her, in friendly competition with the gardener who, like many others of his tribe in those days, was somewhat of a poet and a philosopher. For

George it was an idyllic childhood; he became and remained a keen gardener, and his familiarity with the famous golf course at Westward Ho! made him later a keen golfer. His happy relations with his mother lasted all through her long life. She died in 1928 at the age of 86.

His father had ten children: two sons and a daughter by the first wife, and four sons and three daughters by the second: George was the ninth, and the youngest of the boys. None of his brothers took up an academic career, but all led useful and successful lives: the eldest became Managing Director of a large shipping firm in Liverpool and his son Olaf is the well-known writer; another was a successful land agent; a third was a solicitor in Calcutta; and a fourth entered the Army and became a full Colonel. There was no family tradition of any ancestral scientist on either side.

In 1890 at the age of eight he went as day boarder to the United Services College at Westward Ho! a private school founded by a group of Army officers for their sons: Rudyard Kipling had been a pupil and presumably had made it the setting for *Stalkey & Co.** Conmell Price, whom Stapledon describes as a great headmaster, and who had started the school in 1874, had still four more years of service, but George was too young to come under his direct influence—though one of his Speech Day phrases was never forgotten: 'always the late starters are the best enders.' George found the masters 'a drab lot', he was not conscious of deriving any benefit from them, he gained no distinctions and after he left in 1900 to go to Cambridge he had to spend a year with a 'crammer' in London to make sure of passing the entrance examination.

In October 1901 he entered Emmanuel College and took geology, chemistry and botany for the Natural Science Tripos. But he developed no enthusiasm for any of these subjects. He was, of course, well off and under no compulsion to work hard, being intended for the family business; like others in similar circumstances in those days he was there to develop poise and acquire the arts of gracious living. He was mildly interested in geology but not in chemistry or botany, although he attended lectures by Marshall Ward and A. C. Seward, who was his tutor. He records that apart from a certain amount of golf and of botanizing he was somewhat bored and he was not taken very seriously: indeed he relates that Seward treated him rather as a joke. In the Tripos he got a second class and went down in June 1904, undistinguished and not conscious of any marked influence of any of his teachers.

Three months later he entered the family firm on the Suez Canal and was well trained in the keeping of detailed and accurate records, which proved invaluable to him in later years. But he was not happy; in fact he hated it; he retained only one pleasant memory: a holiday in the Lebanon where he did some geologizing. After two years he decided he could stand it no longer; his grandiose ideas of business expansion were not encouraged, the climate did not suit him, and he wanted to go back to Devon and grow fruit. Very wisely his father agreed: in 1907 he became a pupil on a large fruit farm in Kent. It was an interesting period. Wye College had for some 13 years been making scientific studies of fruit problems and the best of the growers were showing a lively interest therein; Stapledon clearly felt the stirrings for he relates 'it was there I suddenly came to life and realized that I was really a potential scientist'. He wrote for advice to his old College tutor, Professor Seward, who advised him to return to Cambridge and take the diploma course in Agriculture. This he did in October 1908. The course then normally lasted two years, but as the first year was devoted mainly to the pure sciences which he had already taken in his Tripos he was allowed to proceed direct to the second year lectures. It was a youthful vigorous school and much good scientific work was being done. T. B. Wood, who had devotedly tended it since its small beginnings in 1894, had at long last in 1906 been appointed Professor of Agriculture and was introducing into the science of animal nutrition new concepts based on Emil Fischer's work on the proteins, Gowland Hopkins's studies of the vitamins, and Kellner's calorimeter methods of investigation. R. H. Biffen, under the stimulating influence of W. Bateson, was applying Mendelian concepts to the breeding of wheat and had already produced his famous variety Little Joss; while F. H. A. Marshall was continuing investigations on animal reproduction which he had begun under Cossar Ewart in Edinburgh. On the University farm experiments on seed mixtures for grassland begun by T. H. Middleton were still continued, as were S. F. Armstrong's pasture surveys.

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* The school was later amalgamated with the Imperial Service College, Windsor.

But whatever may have been Stapledon's hopes and anticipations before he came to Cambridge none of this appealed to him. He was of course much above the average student age, being now 26, which would hamper student intercourse though it did not prevent friendships with a few who afterwards rose to distinction. Others, however, found him 'very ordinary, with an annoying trick of arguing with anybody and propounding odd and often absurd theories to the senior people as well as criticizing their teaching methods', as one of them wrote. Looking back years afterwards he himself considered his time at Cambridge 'a rather colourless interlude'; forestry ('which I read as a bye-play') was the subject that most appealed to him: the reader was Dr Henry, an amusing and eccentric Irishman 'whose enthusiasm thrilled me', and he writes: 'I was much more deeply influenced by (him) . . . than by any other professor, teacher, or master under whom I had ever sat.' Excepting Dr Henry, he wrote, 'I made no inspiring contacts with my professors and teachers.' Neither at school nor at the University had formal class instruction seemed to do much for him; he felt he gained more from his mother's mode of self education. He took his Diploma in June 1909 and left Cambridge, undistinguished, and having apparently made as little impression as when he had gone down five years earlier. But although he did not realize it at the time, Cambridge had given him two gifts of vital importance for his future work: its technique for analyzing botanically the herbage of a field, which he could modify to suit his requirements; and an ecological outlook, which he may have derived from Dr Henry, as his first ecological references dealt with woodlands.

Immediately after leaving Cambridge he proceeded to the Royal Agricultural College at Cirencester where he had been appointed Assistant in the Department of Natural History, which included botany, zoology, and geology. The Head of the Department, J. R. Ainsworth Davies, who was also Principal of the College, had long been Professor of Biology at the University College of Wales, Aberystwyth, in the days when that title included those three subjects. They had now grown beyond that stage, and Ainsworth Davies, wanting to split off botany, set it up as a new department which was handed over to Stapledon. His first assignment was to help the Forestry Department (then his chief interest) by examining the ground flora of their experimental field, but he was quickly attracted to the field experiments of Edward Kinch, the Professor of Chemistry.

Years afterwards Stapledon said that he owed the beginning of his interest in grassland to Kinch, the outstanding figure on the staff at that time, and the type of man to whom Stapledon would certainly be drawn. He was a native of the region, born 61 years earlier at Faringdon only 18 miles away, soaked in knowledge of country lore and much else—'ask Kinch' was usually the answer to a difficult question—'a fascinating companion on a country walk' as one of his pupils, Lord Bledisloe, described him, also a keen gardener. Mellowed by a great sorrow in early days, when after a year of happy marriage he had lost his young wife, he was deeply interested in his students and young colleagues and always ready to help them. He had first come to the College in 1869 as Assistant in the Chemical Department, but left four years later for a short spell in London; then in 1876 he went to Japan as Professor of Chemistry at the Imperial College of Agriculture, Komaba, Tokyo. Westernization was only just beginning: he had known the old Japan and had brought back with him some examples of Japanese art and crafts which he delighted to show his visitors. He had returned to Cirencester in 1881.

He was well versed in the Rothamsted investigations and had in 1888 started a repetition on a smaller scale of the Park Grass experiments on the manuring of permanent grass for hay, the same treatment being given year after year to each plot so that the effects were cumulative. At Rothamsted not only were weights of hay taken, but botanical analyses were made also. Hitherto this had been impracticable at Cirencester and Kinch now asked Stapledon to undertake it.

This was the beginning of his association with grassland.

At Kinch's suggestion he widened the scope of his enquiries by analysis of the herbage of other Cotswold pastures,* and fortunately for him the two seasons in which he did this were sharply contrasted, 1910 being fairly normal while 1911 was exceptionally hot and dry. Several important generalizations emerged from these observations. Each of the hay plots had developed its characteristic flora, as at Rothamsted, the species most favoured by the treatment having repressed those less favoured. This effect of competition had already been discussed by A. D. Hall. The flora finally established remained unchanged so long as the conditions remained unchanged, but it shifted to a new equilibrium if the conditions altered.

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* He used a modification of the Cambridge method. A frame one foot or alternatively six inches square was laid on the grass, and note was taken of the number of different species and their names, or the number of individual plants or of tillers of each species. The sampling was systematic, not infrequently 100 would be made in a field.

These two factors, competition, and the dominating effects of environment (including treatment) explained several well-known observations. Competition affected not only the components of the flora, but also explained why poor grass lands carried far more species of plants than rich ones: nothing could grow very vigorously and therefore any plant arising from seed blown in had a chance of survival, while on the richer grass land the favoured species grew so vigorously that only equally vigorous intruders could hope to succeed. Stapledon found some twenty to thirty species on the good grass lands, but on the poor land he found over sixty.

But he was far more interested in the ecological problem, the effects of environment in determining the vegetation type. The first requirement was ability to classify the types, and he thought he could do this if he had sufficient botanical analyses, the species in each case being classified in the usual ecological groups: dominant, sub-dominant, etc. In natural habitats there was good correlation between association and environment; he wanted to find out whether it existed for grassland, an association created artificially and then for a period of years left undisturbed except by the grazing animal —the history of most of our permanent grassland. 'It only needs to be proved', he wrote, 'that the flora of an area is essentially a function of the environment, and that chance is not a serious disturbing factor, for ecology to become a very important economic subject . . . a knowledge of the one affords material for accurate deductions concerning the other.' Years later he expanded this theme in connexion with his visit to Australia. In these associations he thought the weeds would be particularly useful as diagnostic agents.

The most important of these observations, however, was made in the hot dry season of 1911. There were heavy casualties among the sown grasses in the pastures, but unsown indigenous plants of the same species had survived much better. One grass in particular attracted his attention: Hard fescue (*Festuca duriuscula*). In the 1913 paper he writes: 'It is probable that, if large amounts of seed from the endemic variety could be collected, results as satisfactory as those recently obtained by the use of wild Dutch (i.e. white) clover might be forthcoming.'*

The search for indigenous varieties, and their utilization for the production of improved varieties of grasses and clovers, became the main purpose of the great Welsh Plant Breeding Station that he later built up. The numerous observations of records provided him with material for several important papers.*

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* *J. Agric. Sci.* 1912-1913, **5**, 129-151. The reference is to D. A. Gilchrist's demonstration at Cockle Park from 1906 onwards of the striking superiority of wild white clover over the cultivated varieties. Years afterwards Stapledon declared that he had 'avidly absorbed' Gilchrist's ideas which, as his study of grassland extended, stimulated his interest in the difference between the indigenous strains of grasses and clovers and those imported from the Continent and Commonwealth. (v. Cecil Pawson, *Cockle Park Farm*, 1960, p. 27.)

He was obviously happy at Cirencester. He was much liked by the students and mingled with them a good deal, joining in their tennis, concerts and other activities: 'he seemed too young', one of them wrote, 'to be regarded as a don to be revered'. He would put up a notice to the effect that on Sunday afternoon he was going for a country walk and any student that would like to join him would be welcome. These walks became very popular and the students were greatly impressed by his power of keen observation, 'with his eyes on the ground to identify every blade of grass, even when not in flower'.

He stayed three years at Cirencester: they were the crucial years of his life: it was there that he found his life's work. He had been a late starter, but he came within the spirit of his old Headmaster's well-remembered dictum and was one of the best enders.

A much wider field opened up for him when C. Bryner Jones, Professor of Agriculture at the University College of Wales, Aberystwyth, obtained a grant from the Board of Agriculture for the appointment of a botanical adviser. Stapledon was chosen. His main task was a botanical Survey of Central Wales: this was expected to take several years. He had also to advise the farmers on botanical matters, and to devote not more than 100 hours per session to teaching in College. There was as yet no Department of Agricultural Botany, and for administrative purposes he was to act under the direction of the Professor of Agriculture, but in matters of research the Professor of Botany had to be consulted. The salary was to be £200 per annum for the first two years, rising thereafter to not less than £300. He took up his duties in October 1912.

He proceeded energetically with the botanical survey. Fortunately for him O. T. Jones, then Professor of Geology, and a group of students were engaged in a geological survey of the region. Stapledon camped with them and made long expeditions on foot into the hills, collecting many sod samples which in the evenings he tore to pieces, identifying and counting the plants, then calculating the percentage that each species contributed to the total. Like some other biologists of his day† he was not strong in mathematics, and his companions were sometimes amused at seeing him use the multiplication table in these operations. These and his earlier analyses laid the foundation of his unrivalled knowledge of the herbage plants, and also provided for the first time a basis for the classification of grass lands which he had been seeking. His maps of the distribution of his vegetation types‡ showed distinct correlations with the geological maps, the governing factor being apparently the water relations.

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* *Sci. Bull. Royal Agric. Coll.* No. 2, 29-36, 1910 and No. 3, 34-43, 1911; *J. Agric. Sci.* **5**, 129-151, 1912-13, and **6**, 490-511, 1914 (effect of manures).

† Especially the seniors. Gilbert would not allow the use of logarithms at Rothamsted and some of the older geneticists distrusted the incursion of statistical science into their subject. Bateson much preferred the 'sieves of a trained judgement'. (Pres. Address Brit. Assoc. Section D, 1904.)

‡ He described some of these in 'Plant communities of farm land'. *Ann. Bot.* **30**, 160, 1916.

These expeditions brought him into contact with the Welsh hill farmers whom he soon learned to like, and they liked him. But he found their agriculture deplorable. Oats were the chief crop, but the sorts grown, he declared, would be classed by most agriculturists as weeds. The other crops were poor, too. Part of the trouble arose from the very poor quality of the seed supplied. He went thoroughly into this question and produced a scathing report on the local seed trade, which led to a speedy improvement.

Many of the hill grazings were very poor,* carrying on the average no more than one sheep to the acre even in summer. He resolved to improve that, and in collaboration with Abel E. Jones he published in 1916 the first of a series of studies which continued throughout his working life.

In 1914 he became acquainted with one of Professor Yapp's Honours Botany Students, T. J. Jenkin, who, quite independently of Stapledon's work, had based his Honours Thesis on some interesting observations he had made on the changing flora of a sown pasture on his father's farm in Carmarthenshire. He had noticed the superiority of self-sown indigenous *Festuca duriuscula* over the commercial variety and had suggested that it should be cultivated to provide a seed supply for farmers. As Stapledon had in his 1913 paper been thinking along the same lines (except that his suggestion was the collection of the wild seed) he proposed to Jenkin that they should join forces and produce a joint paper on indigenous plants in relation to habitat and sown species. This appeared in 1916, and is one of the most important on the subject.† The commercial foreign varieties may give better yields for a few years, but they are sooner or later ousted by the indigenous varieties; it is therefore better to start with these, or even simply to make the conditions more favourable for them when they will more speedily develop of their own accord—an alternative that he had advocated in his 1915 lecture to the Agricultural Education Association but discarded later when the improved varieties of grasses and clovers became available. A list of the plants that should be studied is given.

This investigation was the beginning of a collaboration, which, with a short break early in the 1914-18 war, lasted all of Stapledon's working life.

At first the war had not interfered with Stapledon's activities, but by the end of 1916 the food situation was becoming desperate and a special Food Production Department was set up to increase the home output to the utmost. Stapledon was seconded from Aberystwyth and made Director of the Official Seed Testing Station long intended but never yet established, now, however, to start operations in the Department's offices. It was badly needed, as supplies of seed presented difficulties; it proved so useful that in the first year some 14000 samples were tested.

The Food Production Department was disbanded when the war ended

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* Described in *The sheep walks of mid Wales, chief types of vegetation*, Aberystwyth, 1914.

† *J. Agric. Sci.* **8**, 26-64, 1916.

(November 1918), but the Seed Testing Station was retained. Stapledon, however, returned to Aberystwyth, attracted strongly by a new development. In the struggle for more home-grown food the Welsh farmers had been severely handicapped because the modern improved varieties of crops that served so well in the lowlands were unsuited to their higher altitudes and higher rainfall. A great benefactor of the College, Sir Laurence Philipps (later Lord Milford) gave £10 000 for the establishment of a Welsh Plant Breeding Station and an endowment of £1000 a year for the first ten years. Stapledon was appointed Director and his college post was raised to an independent Professorship.

Before taking up the appointment he visited Denmark and Sweden in company with R. H. Biffen and a few others to see their well-equipped stations for seed control and plant improvement, and he made some useful contacts, notably with Turesson, whose studies of the wild grasses and dicotyledons of Sweden had shown that one and the same species might be found in somewhat modified forms in different environments while still remaining in the same morphological taxa. This work was then attracting considerable attention among botanists.

On returning to Aberystwyth in 1919 in his new dual capacity he found there was no room in the College. A new building was out of the question, but a disused foundry in the town was bought, gutted and converted to provide accommodation for the Agricultural Faculty, and here his new Department was housed for a number of years. Land was acquired about two miles outside the town for the Plant Breeding Station. Work began in May 1919, the staff consisting of Stapledon, one research assistant, and somewhat later, a gardener from Kew. These slender resources were, however, soon augmented: the Development Commission recognized the Station as being eligible for their grants on condition that its work on grass and herbage plants should not be restricted to Wales. This was agreed and in 1920 expansion began. As Senior Scientific Officer he appointed T. J. Jenkin; the junior staff was built up from students, many of whom like Martin and Iorworth Jones came from small Welsh farms, a fertile source of good human material. Stapledon had the gift of choosing his staff well and inspiring in them a deep sense of loyalty and his own vivid enthusiasm for the work they had undertaken.

Having selected a man he assigned him a section of the work, discussed a programme with him, and left him to get on with it, though being always ready with help and advice. The method answered well; it produced men who today are leaders in the particular branches in which they had started at the Station.

The initial programme of work fell into two broad divisions: a search for possible new crops to diversify and enrich the rather meagre systems of farming practised in Wales; and the improvement of crops already grown. The search for new crops yielded little result: none of those tried proved more useful than those already cultivated. On the other hand the efforts at crop improvement were highly successful. Three groups were investigated:

grasses; clovers and lucerne; and cereals with some green fodder crops. The work on cereals soon produced results. It was chiefly on oats, being much the most important arable crop in Wales, occupying nearly half the cultivated land, and was in charge first of C. V. B. Marquand, and then of E. T. Jones who later became Director of the Station. The varieties of *Avena sativa* and *A. strigosa* used by the Welsh farmers were poor, but reasonably tolerant of their conditions. There was already an established technique for selecting and breeding cereals, and after some years of trial and error new varieties were produced, each more productive and better suited to its appropriate conditions than the older sorts. These were well received at the time and two varieties of white winter oats, among the first available to farmers, proved very popular in England; but they have now been superseded by the new varieties bred at the Station.

Clovers and grasses, however, were entirely novel subjects for plant breeders and new techniques were required. The methods suitable for cereals were not directly applicable to grasses because, while cereals are self fertilized, grasses are in the main cross fertilized, the pollen usually being carried by wind. Some varieties,* however, have a certain degree of self fertility and some plants are male sterile.

The first step was to make a collection of as many plant types of the wanted grasses and clovers as could be found. Within any limited and long undisturbed ecological environment the majority of the population was expected to be ecotypical. A wide range of genes within a species would be found only over a sufficiently large range of contrasting ecological habitats. The collection had begun modestly in 1915 when samples of the fine-leaved fescues were collected and studied. But the programme was quickly widened. Both Stapledon and Jenkins travelled widely to survey grasslands, often taking small sample sods as they went; they did this also wherever either of them found on a long undisturbed site an interesting specimen of one of the dozen grasses and clovers they intended to study.† These samples were sent to Aberystwyth, the plant or tillers teased out and planted first in boxes of sterilized soil, then transplanted in rows each of ten plants two feet apart. These were then divided into two sections: one was cut each year for hay and aftermath, the other was cut monthly to simulate the effect of the grazing animal. Observations were made on characters of agricultural importance: habit of growth (leafy or stemmy), length of growing season (time of flowering), winter hardiness, rate of mortality, susceptibility to disease; in selected cases T. W. Fagan of the Agricultural Department determined the mineral constituents and nitrogen content of the leaves.

The collection became enormous. Stapledon called it a 'vast museum' and the number of observations was colossal. In one investigation on red clover R. D. Williams writes that he had made individual observations on ten different characters in 30 000 seedlings over a period of three years.* Stapledon's training in recording in his father's counting house at Suez must have proved invaluable. It was of course magnificent material for an ecologist; A. G. Tansley naturally became very interested and although not a frequent visitor he gave much valuable help over many years, which Stapledon thankfully acknowledged. As an Associate Member of the British Vegetation Committee he met other ecologists and widened his own interest in the subject: his paper on *Cocksfoot ecotypes in relation to the biotic factor*† is one of the classics of agricultural botany. Turesson came also from Sweden and spent a week in the early days: 'One long stimulation' is Stapledon's record. Bateson's visit was an unforgettable event: 'He thrilled all of us . . . generous of his appreciation and of critical advice.' These visits helped to break down the isolation he might otherwise have felt; he took little active part in the work of scientific societies and rarely attended scientific meetings. A visit to the United States in 1923 gave him the opportunity of seeing something of their work on plant breeding, but it was an oppressively hot summer and he was unable to complete his programme.

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* At first these were called 'strains', but as the word 'variety' was already in use for other plants it was later adopted for herbage plants. The Welsh Plant Breeding Station also uses the word 'cultivar'.

† Cocksfoot, Timothy, Meadow fescue, Perennial rye grass. Tall oat, Tall fescue, Crested dog's tail and Rough stalked Meadow grass. The two last named were given up after a few years, partly because they showed less variation than the other species.

During the 1920's British agriculture was passing through long periods of depression and as usual the part of the farm that suffered most was the grassland. Much of it was in a deplorable condition and Stapledon felt that improvement was desperately needed. He was particularly anxious about the hill grazings and unceasingly sought methods of improving them. He repeatedly acknowledges valuable help and facilities given him by two enterprising landowners, Mr S. M. Bligh of Cilmery Park, Builth Wells—'A very volcano of ideas and a pioneer practitioner in the art of land improvement' Stapledon called him—and Capt. Bennett Evans, who owned much land in the Plynlimmon hill region. Beginning with the high land already in cultivation, but where the grass was in very poor condition, Stapledon had for several years investigated two general methods of improvement. One, the ecological method, was to make the conditions as favourable as possible for the better herbage type so that it could readily develop. Given time it would come automatically, the indigenous varieties being widespread. Wild white clover and a few of the grasses, notably rye grass, were the desirable basis and the requirements for clover satisfied the grasses. A dressing of phosphate, either basic slag or mineral phosphate (as Somerville's much publicized 'Manuring for Mutton' experiments had shown) and in some cases lime, together with a good harrowing, often sufficed. But it was quicker and better to plough up and reseed,‡ and from the outset he had tried to devise suitable seed mixtures. Early experiments with clover seed imported from various countries showed the superiority of the indigenous varieties and this proved generally true of grasses also. The numerous trials with seeds mixtures made from 1915 onwards were summarized in 1925.*

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* *Bull. Welsh Pl. Breed. Stn.* Ser. H. No. 2. 1927.

† *J. Ecol.* **16,** 71-104, 1928.

‡ *J. Min. Agric.* **32,** 15-25, 1925.

The higher land, and land that could not be ploughed, was torn up by heavy harrows or other devices and then sown:† 'scratching' Stapledon called this process, but it was rather more than that and required powerful tractors, as also did the strong ploughs when it was decided to use them. These experiments were all on a small scale; they showed what was possible, though not what was practicable from the economic point of view.

Unfortunately early in 1926 he suffered a serious breakdown in health, and his half-brother, who was Chairman of the Blue Funnel Line, arranged for him and his wife to make the long sea voyage via the Cape to Australia and New Zealand, and to stay there for some months. They sailed at the end of February and did not return till mid-November, the College having given him a year's leave of absence.

It was for him a particularly interesting time to visit New Zealand. For long the natural fertility of the soil had been failing, and by the early 1920's the position had become very serious. E. Bruce Levy had been appointed to find ways of improving the grasslands and had already done valuable work, mainly on ecological lines. Stapledon fully recognized the importance of these factors, but insisted (and he could insist with great vigour) that 'strain', or as it is now called 'variety', could outweigh their significance and even nullify the agronomic value of any soil improvement. 'He led New Zealand', writes Bruce Levy, 'to a thorough realization of the significance of strain and pedigree in herbage plants, and set in motion intensive strain trials of agronomic strains of grasses and clovers . . . Strain and pedigree in herbage seeds in New Zealand have been among the most important factors in our development of marginal lands and in the surer and fuller intensification of our grassland farming systems.' So impressed was the Department of Agriculture with the possibilities thus opened up that Stapledon was asked to send out one of his staff to continue the trials. This he was able to do later, and he chose one of his ablest young colleagues, William Davies,‡ to work as Plant Geneticist at the Palmerston North Research Station, where he did such good work that on the occasion of the 7th International Grassland Congress in 1956 the University of New Zealand conferred upon him its Honorary D.Sc. degree.

But if Stapledon gave much to New Zealand he also received much. In the rich dairy districts of the North Island he saw more intensive management of grassland than anywhere in Britain. Instead of having the grassland in large fields each open for grazing over the whole area for the whole season it was divided into small paddocks each grazed only when at full vegetative growth, then fertilized and rested till full growth was again attained. This effect of resting and grazing in enhancing productiveness interested Stapledon greatly and was further studied at Aberystwyth when he returned.*

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* *Advisory Bull. Univ. Coll. of Wales*, with William Davies, who was put in charge of this work in 1923.

† Stapledon made much use of the cleanings of wild white clover seed: sufficient clover was left to establish the plant and the foreign seeds would usually do no harm even if the resulting plants survived on the hills, while some, e.g. *Lotus corniculatus*, would be useful (v. the 1915 lecture).

‡ Now Director of the Grassland Research Institute, Hurley, Maidenhead. After two years in New Zealand he spent a year in Australia to inspect the grassland experiments in each State and report thereon to the Commonwealth Council of Industrial and Scientific Research.

In Australia he saw the various stages in the conversion of the native flora—the stable climax vegetation—into the unstable transient forms that dominated the countryside. 'No knowledge', he wrote, 'could be more essential to the national exploitation of a country than a proper appreciation of the relationships—successional relationships—of the different types of vegetation to each other and to the varied soil and climatic conditions which obtain.' This knowledge would facilitate the conversion of the natural vegetation into a more desirable type and indicate ways of maintaining it.

What impressed him most in this journey, however, was the enormous potential but unrealized value of the grasslands of Australia, New Zealand and such of the South African veld as he had seen during the ship's stay at Cape Town. None of these countries had a properly equipped grassland research station, and he recommended that one should be established on generous lines like the Svalöf Station in Sweden; also that in each country proper agrostological surveys should be made.†

He returned to England much improved in health and found a lively interest in agricultural development both at home and in the Commonwealth. The first Imperial Agricultural Conference had been held in Westminster Hall in October, and was just ended; its recommendations had included the establishment of Imperial Agricultural Bureaux‡ for the dissemination of information on their respective subjects to agricultural departments throughout the Commonwealth. One of these dealt with grass and forage crops, and was to be located at the Plant Breeding Station with Stapledon as Director. His proposal for the establishment of a great research institute was not accepted, however.

At home he found a greater interest in grassland than ever before. The Ministry's campaign for its improvement was being vigorously prosecuted and he thought the time was ripe for him to issue a book on the subject for the guidance of farmers; to widen its scope he collaborated with J. A. Hanley, one of the most experienced Agricultural Advisers of the time. They wrote *Grassland, its management and improvement* (Oxford University Press, 1927). It was well received, but the depression of the early 1930's disheartened farmers and they lost interest; when finally the tide turned the Second World War was on, and reprinting was impossible.

Unfortunately in 1928 he had another nervous breakdown the effects of which lasted a long time: nevertheless he continued to work as much as he possibly could. Meanwhile some of the plants in the Station's collection appeared sufficiently promising to justify extended farm trials, and by 1930 selections of grasses, clovers, and oats were being tested throughout Britain.

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* *J. Min. Agric.* **34,** 201-212, 1927. Martin Jones and Iorworth Jones, *Bull. Welsh Pl. Breed. Sta.* Ser. H. No. 11. 1930.

† *A tour in Australia and New Zealand: Grassland and other studies,* Oxford Univ. Press, 1928; also papers in *J. Min. Agric.* **34,** 1927.

‡ Later renamed Commonwealth Agricultural Bureaux.

Stapledon realized that selection alone would not provide the range of characters wanted; combinations of characters were required which could be obtained only by breeding. Fortunately the search for suitable techniques had been successful, after what he described as 'eleven years of trial and error', and the first results were published in 1931.* Following his usual practice he had alloted the grasses mostly to T. J. Jenkin, the clover to R. D. Williams, and kept Cocksfoot (*Dactylis glomerata*) for himself. He chose Cocksfoot because in the Aberystwyth region it occurred more commonly than any other grass in more or less well-defined ecotypes, it was also more highly self fertile than most of the other local grasses, and, as one of the most persistently leafy of grasses, it had high agricultural value. It soon appeared that the pure line technique was unsuitable, there being too much heterozygosity. Recourse was therefore had to mass hybridization. About 30 desirable parents were selected from the 'vast museum', they were selfed, some 200 'quintessential plants' were selected from the progeny but not emasculated, they were enclosed together in conditions intended to exclude foreign pollen, and at the proper time shaken by a simple mechanical device to ensure distribution of the pollen. In each generation there was re-selection, and non-conforming plants were rejected. The method was frankly empirical and subjective, 'largely selection and trial on what would now be termed polycross lines', a colleague described it.

Stapledon was not interested in genetics or cytology. At the outset he disclaimed any intention of aiming at genetical purity. This he considered might be necessary for crops like wheat that have to be processed, but not for forage crops where the difference between higher and lower purity is chiefly a difference in weight per unit area rather than in quality. Many generations, he wrote, would be needed to attain complete genetic purity, whereas a 70 per cent purity could be attained much sooner and would represent a great advance on existing varieties of grasses. It was not that he was satisfied with 70 per cent purity: he was imbued with a deep sense of urgency, the pressing need to improve the grasslands of the country, especially in the hill districts, and as the new varieties were a great advance on those then in use he wanted to get them out to the farmers as quickly as possible. Like some other very successful plant breeders he had a clear vision of what he wanted to produce, and the artist's intuition to guide his choice of initial material.

The method is open to the objection that some degree of heterozygosity is inevitable, rendering the variety liable to instability: indeed at one time there was a blurring of the differences between certain varieties, but this was rectified by reconstructing them. His judgement had, however, been so good that his first strain of Cocksfoot to be distributed, S. 26, developed from five plants found in a gorse brake in Devon, and put on the market in 1937, still retains its popularity as do two later ones, S. 37 and S. 143.

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* *Bull. Bur. Pl. Genet. Aberystwyth*, **3**, 1931. R.G.S's section is on pp. 35-45.

While Stapledon was producing the Cocksfoot varieties, T. J. Jenkin was working on Perennial Ryegrass (*Lolium perenne*), Timothy, and other grasses with equally successful results: some of his strains, especially S. 23 rye grass, are still very popular. His technique, however, differed from that of Stapledon, being based on genetic principles.* So also was the technique of R. D. Williams who produced the very valuable S. 100 white clover and S. 123 red clover.

Considerable time elapses between the production of a new variety at a Plant Breeding Station and the appearance of its seed on sale to farmers. For the market, of course, it must be produced in tons. Grasses being cross-fertilized, with wind as the carrier of the pollen, special care has to be taken to avoid contamination during their multiplication. Anything from 12 to 16 years may elapse between the first crossing and the marketing of the resulting new variety; for the two clovers just mentioned the interval had been 17 years: from 1920 to 1937.

Stapledon did not wait for this final stage: having got sufficient seed of these better varieties of grasses and clovers for experimental purposes he was anxious to organize a large-scale demonstration combining all the methods of improvement of hill grazings that had proved effective in his numerous trials. He drew up an ambitious scheme estimated to cost £20 000, a large sum in those days and far beyond anything the Government would provide. He and the College Principal, H. Stuart Jones, sent a letter to *The Times* pointing out that large areas of grass in our country now regarded as inevitably derelict could, by these new methods, become an asset of immense importance, and appealed for funds to show the way. The letter was published on 11 July 1932: soon came the response by Sir Julien Cahn who generously offered £3000 a year for seven years. A lease was taken of a hill farm near Devil's Bridge with grazings up to 1350 ft altitude, and some 2500 acres of sheep walk up to 1850 ft altitude, much of the herbage being of the very poor Molinia-Nardus type with Nardus-Fescue in the drier places and rushes in the wetter parts. The customary procedure was to keep the sheep on these walks during the summer, but to remove them during autumn and winter, those ready for market being sold, the in-lamb ewes and some others transferred to lower lying fields on the farm and the weaker ewe lambs boarded out on lowland farms between October and early April. This was a costly and often unprofitable business, and Stapledon sought to improve the cropping system of the arable part of the farm so that it could carry more sheep in winter, thus reducing or eliminating the cost of boarding out. His further object was to make the summer grazing more profitable.

Moses Griffith, Agricultural Adviser for Merionethshire, and a good farmer, was put in charge; work began in March 1933, and yielded striking results,† which Stapledon intended should be seen. He had a wonderful gift of presentation. From the road one saw on the hillside a great patch of green herbage dotted with sheep happily grazing, surrounded by the dark wild forsaken vegetation to which, however, they occasionally had recourse apparently in search of some nutrient not adequately provided by the plants sown. When Sir Julien's gift was exhausted the scheme was financed by the Ministry of Agriculture till 1947 when it came officially to an end. The work still goes on, however, the land being now included in the Ministry's Experimental Hill Farm.

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* Described by him in *Bull. Welsh Pl. Breed. Stn.* Ser. H. No. 2. 1924. The method was devised in 1921.

† Reported in *J. Roy. Agric. Soc.* **97**, 33-53, 1936, and *Welsh J. Agric.* 1935 and 1937.

The information obtained has wide and lasting value, and with Stapledon's other work and that of his colleagues it has provided most of the techniques now employed by farmers for upland reclamation. By the irony of fate the grass varieties he himself produced did not turn out to be the most suitable for the purpose: the really pivotal grass, according to P. T. Thomas, the present Director of the Station, is another of its products: Jenkin's remarkable S. 23 rye grass; this, Thomas states, has been responsible for more grassland improvement on the hills than all others put together. But Stapledon's influence and advice stimulated reclamation of large areas of hill land during the war, much of which still remains greatly improved: notable examples are on the Kerry Dolfor hills and the Shropshire hills in the Bishop's Castle and Clun areas.

Meanwhile the experimental plots at the Plant Breeding Station, always interesting to scientists, were becoming increasingly interesting to farmers, and many came from far and wide to see them: usually in groups. Stapledon himself frequently demonstrated to them. At this he was unrivalled. 'Dressed in country tweeds with plus four stockings of unusual hue and a woollen sweater round his waist in the manner of a cricket umpire', as a colleague described him, carrying a shooting stick used sometimes as a rest, sometimes waved about as a pointer, holding his audience spellbound by his vivid descriptions and his amazingly infectious enthusiasm. It even happened at times that the char-a-banc due to take the party home at 6 p.m. would be kept waiting two hours or more because he was still willing to continue. On occasion for large groups at Cahn Hill, where he had taken to riding, he would demonstrate mounted on a Welsh pony armed with a trumpet-shaped megaphone, a striking unforgettable figure with his finely moulded features and great shock of hair: he must often have reminded his Welsh hearers of a crusader of old calling them to battle in tones as near to the *hwyl* as a *Saes* can ever get. And indeed it was a crusade he was leading: an end to the poverty of the hill farmers and a better life for them through more productive grassland.

Improvement of the grasslands, however, was not the only object for which he was campaigning. Like many others in those days sensitive to the beauty of the countryside, he wanted to stop the devastation that was then proceeding at an appalling rate as the result of very heavy wartime losses of sons and fortune sustained by the county families who had long been its custodians. He deplored the growing estrangement of the mass of the people from the countryside, their failure to recognize its beauty, their desire to speed through it as quickly as possible, indifferent to the sprawl of the towns and the unsightly erections being put up in what had been attractive little market towns and villages. In 1935 he wrote *The land now and tomorrow* (Faber & Faber) telling of some of the atrocities he himself had seen, of the unnecessary village poverty due to the neglect of our agriculture, and in glowing terms setting forth what rural Britain could be like if changes now quite practicable were made. But this would require widespread appreciation of the ethical, aesthetic and social values of a highly efficient agriculture and a well-cared-for countryside. His remedy was to bring the townsman into the country and to set up National Parks, where a model village, model farms and rural crafts would show the best of country life to the townsman visitor; and rowing, fishing, riding and simple golf (he despised the modern costly 'super golf') would show him its pleasures.

The book had a wonderful reception: repeated reissues were called for, and it must have played an important part in stimulating public interest in the countryside. The idea of National Parks was of course not new: it had been discussed for many years, and the Addison Committee of 1929 had studied it in detail and recommended certain areas as eminently suitable. Their idea, however, was entirely different from Stapledon's: it was to leave wild nature intact in all its pristine beauty allowing no interference by man, and it was this, and not Stapledon's conception, that was adopted when the National Parks Commission was finally established in 1949.

The book has the strong personal interest of revealing the man more fully than any other of his writings; it brings out the streak of poetry in his nature, the visionary's disregard of the practical difficulties that would have benumbed most others, but which had seemed to melt away before his robust enthusiasm.

Then in 1935 came another breakdown in health which much reduced his activity for many months.

Later on the Ministry of Agriculture wishing to intensify the grassland improvement scheme asked Stapledon in October 1938 to do a lecture tour of England and Wales, and also with William Davies and three assistants to make a detailed survey of the grass lands. His theme for the lectures was that the grass now treated as permanent should be regarded as a crop and made to play its part in the rotation. Instead of being left down indefinitely it should be ploughed up and followed by a highly productive sequence of arable crops. When its turn came the ley should be broken up before it began to deteriorate—though he realized that while it remained it improved the physical condition of the soil and increased its productiveness. Country wisdom on the matter was summed up in the old couplet:

Make a pasture, break a man;
Break a pasture, make a man.

For this he substituted the slogan 'Take the plough round the farm'. As some 60 per cent of the farmed land of England was in permanent grass the task of persuading farmers to bring it under the plough was formidable, but this did not deter him: even the renowned fatting pastures of Leicestershire and Northamptonshire were to be ploughed up. This, however, was too much for the stolid countryman: from time immemorable it had been held that to break these up would be almost a sacrilege. A compromise was necessary, but Stapledon estimated that some three-quarters of the permanent grass could advantageously be replaced by arable grass. Ley farming, he maintained, should form the basis of our agricultural policy, and he had made this the subject of his Presidential Address to the Agricultural Section of the British Association earlier in the year.*

Meanwhile the grassland survey was becoming increasingly important, for war was threatening and if it should break out the survey would form the basis of the inevitable ploughing up campaign to increase the area under corn. It was a remarkable undertaking and the most extensive ever made.† There were three operations: (1) zoning, based on the known relation between botanical composition and colour; (2) classifying, based on qualitative assessment of the herbage of individual fields within sample blocks; (3) botanical reading on a few randomly distributed fields within each area: this provided a common standard for the surveyors concerned.

The survey showed that more than 9 million acres of permanent grassland were 'a standing reproach' but 'fit for the plough'. Much of it had been ploughed up and made productive in the first war, but during the post-war depression had deteriorated or even become derelict. The idea that the grass should come into the ordinary rotation was of course well known and was common practice in the north and west of the country, but the new herbage plants produced at the Station had greatly widened the possibilities. By sowing one field with a late developing ley and another with an early developer it became possible to extend the grazing season which on current practice had lasted some six months only. This problem of extension is still being studied by William Davies and his colleagues at the Grassland Research Institute at Hurley; he is optimistic enough to believe that it can ultimately be extended to cover the whole year.

When war broke out in October 1939 Stapledon wished to have a large scale demonstration of ley farming as a means of rehabilitating the extensive areas of heavy clay soil then more or less derelict in the lowlands just as the Cahn Hill scheme had demonstrated the possibilities of reclaiming waste hill land. The Ministry of Agriculture agreed, and provided funds for a Grassland Improvement Station at Drayton, near Stratford-on-Avon, on a derelict 500-acre farm, much of it on heavy Lower Lias clay: 'the epitome of all that is wrong with rural Britain' was Stapledon's description.‡ Two other equally dispiriting farms were included in the scheme, 1300 acres of poor moorland in Staffordshire, and 1000 derelict acres on the Cotswolds. Stapledon was in charge of all of these, and the changes he effected attracted large parties of farmers; he also did much lecturing to large farmer audiences besides retaining his post at Aberystwyth. He was fortunate in securing William Davies's help, for war-time difficulties with labour and transport made the combined tasks very heavy, grossly overtaxing his strength.

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* Later expanded into a book *The plough up policy and ley farming*, Faber & Faber, 1939. In order further to popularize the system he and William Davies later (1941) wrote a Penguin book entitled *Ley farming*.

† Mimeographed in 61 parts in 1940.

‡ *J. Roy. Agric. Soc.* **109**, 17-27, 1941. The programme of work is given here.

In 1942 he felt no longer justified in retaining the Directorship of the Plant Breeding Station in view of the long absences necessitated by his work for the Ministry from 1938 onwards and the improbability of any relief. He therefore resigned so as to be freer to continue his ley farming demonstrations.

At the end of the war in 1945 his work ceased and he retired into private life. Not, however, to rest and recoup. As with other scientific workers of those days the pre-war pension arrangements were quite inadequate to meet the rising costs of post-war years and there followed a period when he had to do some journalism. In 1947, however, Messrs Dunn's Farm Seeds of Salisbury invited him to become their Scientific Adviser and, later, one of their Directors. This brought him back to his favourite occupation in very congenial circumstances. Messrs Dunn's were old friends; they had been among the first to recognize the importance of the Station's work and to distribute the new varieties of grasses and clovers to farmers. So long as his health allowed he worked very happily with them.

Then began the pitiful tragedy of his last years. In 1952 he had a serious operation, and from then on to the end he was rarely free from illness: angina, kidney trouble, bronchitis each winter, Menière's disease, and then, greatest affliction of all, stone deafness for the last three years of his life. All communication with him had to be by writing. To the end, however, his mind remained unclouded, restless and critical as ever. His interest in agriculture waned but was replaced by a wide range of others; among them poetry, philosophy, psychology and education. He left twelve large note books full of comments and ideas on what he had read. Proust, Rilke, Kirkegaard, Ortega, Arthur Koestler, du Chardin, were among his favourite authors which he enjoyed discussing with a new-found friend whose visits used to lighten the tedium of his long and often painful illness.

He died on 6 September 1960. Tributes to his work poured in from farmers and many others. The B.B.C. organized a programme on the Welsh Service 'I remember Stapledon', and eight months later another one on the Home Service, in each of which a group of speakers took part. Some of his friends have established a Memorial Trust to ensure that his memory shall be perpetuated.

Few men in his generation left a greater mark on the countryside of Britain than he did. Before he began his work grass had been the Cinderella among crops: it could tolerate neglect and in bad times was the first to get it. Great stretches of permanent grass had a woebegone appearance, and produced only a fraction of what was possible: it was a thing apart from the cropped land.

He made it possible to effect a complete integration in all but the drier eastern parts of the country, thus considerably changing the landscape of the farm and giving the greatly increased yields of grass that have been so important a factor in bringing about the recent marked increase in output of milk and meat. His work on the reclamation of hill land shows how large areas of the 5½ million acres of rough grazing in England and Wales could, if and when the need arises, be made much more productive; it has also enabled marginal land to be kept in cultivation when otherwise it might slip back and become derelict: counteracting, in short, the tendency of the boundary of cultivation to drop further down the hill.

The varieties of grasses and clovers produced at the Station have proved of great value not only in the British Islands but in Australia, New Zealand, South Africa and elsewhere.

His scientific work lay in the border land of ecology and the agricultural group of applied sciences, especially of the new applied science of grassland agronomy of which he was one of the chief founders, systematizing much scattered and fragmentary empirical knowledge and building it up into a new subject which, infused by his successors with modern principles of genetics and cytology, promises to attain great importance. He had an unrivalled knowledge of the grasses and of their properties, and the gift of intuitively finding a short cut to the solution of a problem that otherwise might have required long investigation. His ecological outlook enabled him to link up the grass and the land, and to ensure a sound practical outcome from his labours.

Recognition in abundance came to him from widely different quarters. He was early awarded the Honorary Membership of the Highland and Agricultural Society and the Swedish and old Czechoslovakian Academies of Agriculture; in 1932 he received the C.B.E. and in 1939 he was in rapid succession made a member of the Athenaeum under Rule 2, elected a Fellow of the Royal Society on the first application by his sponsors, was awarded the Gold Medal of the Royal Agricultural Society for distinguished services to Agriculture, and was given a knighthood. Unfortunately, however, overwork and strain had again incapacitated him and he was unable to go to Buckingham Palace to receive the accolade: it was bestowed by Letters Patent. University honours came later: after the war the Universities of Nottingham (1951) and of Wales (1952) each conferred upon him the Honorary degree of Doctor of Science. The University of Durham had in 1958 offered its honorary Doctorate of Science which unhappily he could not accept, as by that time he was unable to make the journey to Newcastle where it would have been conferred.

He had the gift of being able to pick out as colleagues young men of marked ability to aid him in his work and of retaining their loyalty and affection for life. In this he was helped by his abiding interest in young people. Starting with only one assistant he built up with their aid a great research station with an enviable and world-wide reputation and which has trained a succession of young scientists doing valuable work in their subject in many parts of the Commonwealth.

He was not interested in College organization as such, nor in Faculty or Senate meetings; indeed he seemed completely oblivious of any regard for academic procedure or regulations, and was often rather a rebel, but a likeable one. Nor did he take much part in meetings of scientific societies except the Grassland Society of which he was one of the founders and its first President: its first Congress was in 1937. 'Grass and grassland, and yet more grass and grassland' as he put it, were the subject of his labours; and gardening, golf, and later riding his relaxations. Yet he was no dull recluse: his love of the countryside, his vigorous enthusiasm for it, the visionary and poetic elements in his character with a touch of Peter Pan; his intense dislike of pretentiousness and injustice combined with his gift of racy description made him a good companion popular alike with students and staff in the College—'Stapes' he was to them—and brought him a wide and varied circle of friends outside.

In his 'Personal notes' he wrote: 'No obituary notice of myself would be complete without reference to the constant help I have received from my wife.' She was Doris Wood, only daughter of Thomas Wood Bourne and of Jessie Bourne. They were married in 1913. Having no children she was able to devote herself whole-heartedly to him and his work, which she equipped herself to follow; always, too, she sedulously guarded his uncertain health. They were inseparable companions. His extensive agricultural tours were almost invariably by car and she always drove, he never. Towards the end he estimated that she must have driven him some 700 000 miles, often over very difficult moorland trackways, to almost inaccessible places. In prefaces to his books he describes her variously as his dictionary, his ready reckoner, on occasion his Cahn Hill demonstrator. It is certain that without her aid he could never have achieved anything like what he did.

Science has its unsung heroes, and the world will never know what it owes to the mothers and wives of its scientists.

E. JOHN RUSSELL

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II. Papers.

Stapledon published a very large number of papers, most of them short, written for farmers, and published in the farming journals. Complete lists are filed in the Libraries of the Royal Society, The Welsh Plant Breeding Station, Aberystwyth, and the Commonwealth Bureau of Pastures and Forage Crops, Hurley, near Maidenhead, Berks. Those published between 1919 and 1941, Stapledon's most creative period, are listed in *Bibliography of the Welsh Plant Breeding Station* (1956).

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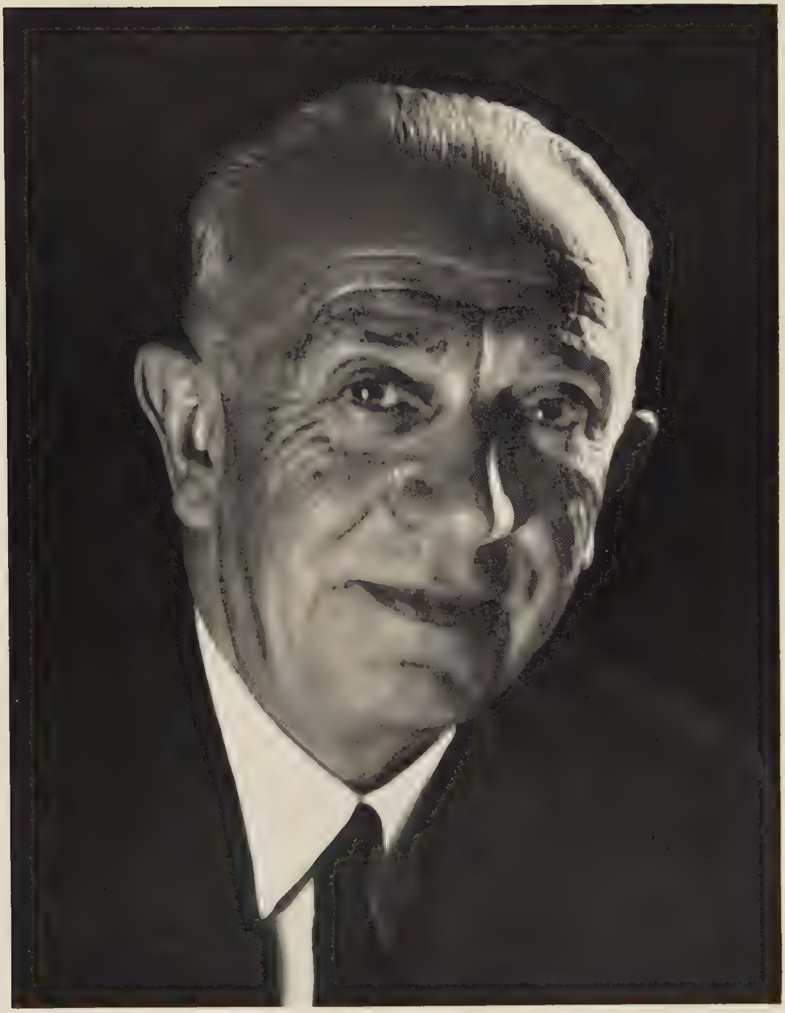

John S B. Stopford

JOHN SEBASTIAN BACH STOPFORD, BARON STOPFORD OF FALLOWFIELD

1888-1961

SIR JOHN SEBASTIAN BACH STOPFORD, FIRST BARON STOPFORD OF FALLOWFIELD, of Hindley Green in the County of Lancashire, of the Life Peerage, was born at Hindley Green near Wigan on 25 June 1888 and died at his home in Arnside, Westmorland, on Monday, 6 March 1961. His father was Thomas Rinck Stopford, a colliery engineer, his mother Mary Tyrer Johnson of Bolton. He attended both Liverpool College and the Manchester Grammar School from whence in 1906 he entered the Medical School of Manchester University with which he was to be intimately connected and the future of which he was to do so much to mould.

The Medical School was in one of its most exciting and fruitful periods. The clinical staff were strong but were matched by the pre-clinicals. William Stirling taught physiology, whilst during Stopford's period as a student Grafton Elliot Smith was appointed to the Chair of Anatomy, too late, perhaps, to influence Stopford at the undergraduate level but to be a most potent influence in his post-graduate studies. Stopford's filial tribute 'The Manchester Period' tells much of the School and of the man.*

Frederic Wood Jones was Senior Demonstrator in Anatomy but much of the teaching fell into the able hands of Dr T. Wingate Todd. Geoffrey Jefferson, Harry (now Sir Harry) Platt and John Morley had entered the school a year or two before but Stopford had among his earliest students, O. M. Duthie, F. R. Ferguson, P. B. Mumford and R. L. Newell—all of whom were to achieve distinction in the sphere of clinical medicine in his own school and hospital.

Stopford graduated M.B., Ch.B. with honours in 1911 winning the Dumville Surgical Prize and the Bradley Memorial Surgical Scholarship at the Manchester Royal Infirmary. A resident post at Rochdale Infirmary followed and in 1912 he joined the Department of Anatomy as Junior Demonstrator under the late Sir Grafton Elliot Smith whom he was to succeed seven years later as Professor of Anatomy. In 1913 he was awarded the Tom Jones Memorial Scholarship in Surgery and in that year the first of his scientific papers appeared. In 1915 he was awarded the degree of M.D. receiving the Gold Medal for his thesis.

In 1916 Stopford married Miss Lily Allan and there is one son of the marriage. Lady Stopford is also a graduate of the Manchester Medical School and developed very considerable interests in ophthalmology until the increasing pressure of public duties compelled her to withdraw from practice. The marriage gave Stopford a firm background of domestic care on which he drew lavishly in the serious illnesses of his later years. It was an ideal partnership though it would be difficult to find two people with so many, at least superficial, differences. He infinitely approachable and almost completely extrovert; she quiet, deeply reserved; he finding his life in a few interests, predominantly the University, then hospitals, medical education, gardening, association football, and more than satisfied with a few days in North Wales or on the Lancashire coast, she with more widely ranging interests and particularly foreign travel, which opportunity, alas, only rarely permitted her to indulge. But she has, fundamentally, a serenity which he lacked. Opposition of any kind, particularly in his later years, took much out of him and his capacity to carry on vigorously to the end owed more to her care than most people ever suspected.

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* *Sir Grafton Elliot Smith.* Ed. Warren R. Dawson, London: Cape, 1938.

Stopford's first scientific papers were short communications on human topographical anatomy, one of which concerned the shape of the normal stomach. This preliminary study led him later to collaborate with E. D. M'Crea and B. A. M'Swiney (of the Department of Physiology, Manchester University) on a radiological investigation of the normal movements of the stomach in man, and experimental studies on the effect on the stomach of section and stimulation of the vagus nerve. But Stopford's interests became rapidly concentrated on problems of applied neurology, and it was here that he made distinguished scientific contributions. His appointment early in the First World War as neurologist to the Second Western General Hospital and the Grangethorpe Hospital stimulated him to pursue his purely anatomical studies into the clinical field, and led to the publication of a series of papers on cutaneous and deep sensibility arising from his observations of peripheral nerve injuries. But his first important contribution was a monograph on the arteries of the pons and medulla of the brain, wherein he demonstrated the degree of their variability and the relation of their distribution to the clinical syndrome resulting from vascular lesions affecting them. It was for this extensive and detailed research that Stopford was awarded the M.D. of his University, with a gold medal, and it is fair to say that the monograph still remains a classic work of reference.

Stopford's work on the peripheral nerves was partly concerned with the treatment of large defects due to wounds, and with the results of secondary suture of severed nerves, but he also published several observations on the effect of local pressure on nerves—particularly the pressure exerted by cervical ribs. But no doubt he will be best remembered for his studies on the recovery of sensation in the course of the nerve regeneration. To some extent Stopford agreed with the observations of Henry Head and W. H. R. Rivers that cutaneous sensibility could be divided into two categories—protopathic and epicritic (or what he called 'the protective and discriminative divisions of sensation'), but he extended this conception to include also deep sensibility. He also developed the view that the two categories (which he thought could be recognized as the successive stages in the recovery of sensation during the regeneration of a cut nerve) were not due to differential rates of growth of peripheral nerve fibres as Head had postulated, but could be more reasonably explained on the basis of successive adjustments in the central nervous system. His demonstration, partly based on dissections and partly on the study of clinical material, that at least some of the so-called cutaneous nerves commonly send fibres to neighbouring joints upset the traditional concept that the superficial and deep innervation of a particular region of the body is fundamentally distinct. Much of this systematic study of cutaneous and deep sensation was incorporated in a small book published in 1930, entitled *Sensation and sensory pathway.*

In collaboration with the Professor of Surgery at Manchester, E. D. Telford, Stopford later made clinico-anatomical studies of the autonomic nervous system, with particular reference to the surgical treatment of vaso-spastic conditions, and he also contributed some interesting reflections on the possible relationship between certain types of raised intracranial pressure and the anatomical disposition of some of the cerebral veins.

Stopford did not publish any scientific papers after 1936, and it is only to be expected, therefore, that some of the conceptions which he developed have now been superseded as the result of more detailed studies since then. But there remains no doubt that, during his scientific career, his work was widely recognized to be of considerable importance, and many anatomists felt that his translation to a life of administrative work (though exceedingly productive in its results) was a severe loss to the subject which he so ably represented in his time.

Scientifically his career must be regarded as frustrated. He showed, far too early, that administrative skill which drew him remorselessly into the wider world of university affairs, first in Manchester, then in the nation as a whole and finally—though the coming of the war truncated this—in the Commonwealth. He was appointed to the Chair of Anatomy in 1919. Within four years he added to his academic duties those of Dean of the Medical School. This post he relinquished in 1927 but the following year became Pro-Vice-Chancellor for the statutory period of two years. In the period of 1931-1933 he was again Dean of the Medical School.

For many years Stopford was the University's representative on the General Medical Council, in which office he succeeded Professor R. B. Wild, and was Chairman elect of that body when his first illness came, thus depriving him of succession to the Chairmanship.

In 1934, the then Vice-Chancellor, Sir Walter H. Moberly, accepted appointment as Chairman of the University Grants Committee. Stopford was appointed a member of the Committee to recommend his successor, but was quickly advised that he should absent himself from the meetings and for the first time in its history the University had the confidence to elect one of its own graduates to be its executive head. Some considerable pressure was needed to induce him to desert his scientific work and his teaching, but he yielded, and took delight in repeating the story of how, walking home, on the day of the election, he met a member of the clinical staff coming out of the teaching hospital. 'Is what I hear true Jock?' 'I suppose so.' 'Well, if that's what you want I suppose it's all right. But I'd rather run a zoo myself.' Fellows of the Royal Society might well care to hazard the authorship of that remark!

No one hearing Stopford speak could doubt his power as an advocate, and the testimony of all his students was immediately confirmed by the experience. There was a simplicity, a directness and a consistency about it all which fitted in with all one knew of the man. He had a hatred of the polysyllabic and the Anglo-Saxon dominated the Latin in all he said. The University of Manchester has always, and quite rightly, been regarded as a research university, and has, indeed, not escaped criticism in this connexion. But Stopford was always very much a 'students' man, and the teaching function of a university was never far absent from his mind—or his policies.

If Stopford's scientific life had been frustrated by the recognition of his administrative skill, there is little doubt that his life as Vice-Chancellor was gravely circumscribed by the war—and the years of restriction and privation which followed. To his successor, basking in plans of extension and the receipt of state grants of a magnitude that Stopford himself could never contemplate, that much seems obvious. Yet it is doubtful whether the notion ever crossed *his* mind. Certainly the years were years of great academic development, particularly in the quality of the senior academic appointments which he influenced. He was an unerring judge of men, and the skill he had shown in appointment committees from 1919 onwards reached new levels during his Vice-Chancellorship. The pretentious and the second rate never escaped him, yet there was an absence of formality at any committee of which he was chairman which put men at their ease immediately. Perhaps fatally so—for he had no little forensic skill in examining a candidate who dropped his guard. It was an experience not to be forgotten to see him take the chair at a Women's Scholarship Awarding Committee. The girls, often immature, even for their age, answering his questions demurely and revealing more of the tastes of their teachers than of themselves; never listening to music less classical than Bach or Beethoven and reading Tolstoy or Dostoievsky in sterner mood with perhaps a Shakespearean comedy in more frivolous moments. Stopford never failed to pierce their armour . . . and his 'But what music do you really listen to on the wireless for pleasure' or 'I simply do not believe that you read Tolstoy in bed at nights' almost invariably evoked a flood of confidences which told him and his committee all that they wanted to know of the candidate.

Had his life fallen out differently there can be little doubt that he would have had great success as a clinician. He had a power of diagnosis and indeed of prognosis which was invaluable at committee and which was invariably respected by clinical colleagues. Many a doubtful appointment was crushed at birth by his faculty of observation. Even restricting oneself to the years in which he was Vice-Chancellor, Stopford played a leading part in bringing to his University, Manson in Biblical criticism, Hartree, Blackett and Rosenfeld in physics, Todd, Hirst, Jones, Evans, Gee and Birch in chemistry, Wood Jones in anatomy, Willis Jackson and F. C. Williams in electro-technics, Rowley in Semitic languages, Goldstein, Newman and Lighthill in mathematics. Of his medical colleagues he took great pride in the creation of personal chairs for Sir Geoffrey Jefferson and Sir Harry Platt, Bt. and in the appointment, in 1945 of Sir Robert Platt, Bt. to the University's first full-time chair of medicine.

He had served on the Inter-Departmental Committee on Medical Schools which was appointed in March 1942, and had been Vice-Chairman of the Committee under the chairmanship of Sir William Goodenough. To the report produced by the Committee he must have made liberal contributions for those who knew his views on medical education found many of them there reproduced. Hence he took particular interest and pride in the creation of full-time clinical teaching appointments in general and regarded the creation of the full-time clinical chairs of medicine and surgery at his *nunc dimittis*. They were in fact far from that, much vigour and activity lay before him but, to the end here mained acutely zealous of the principle of full-time teaching and was watchful of and uncompromising towards any attempt to reintroduce the part-time teacher. As an administrator he was sometimes criticized for inability to delegate, not infrequently, one suspects, by those whom he had not preferred when distributing administrative chores. It is difficult to reconcile the criticism with the facts. Despite a major illness in 1943 which had given great concern to his friends, and with considerable heart searching, the Univerity, under the strongest ministerial pressure, agreed that Stopford should become the first Chairman of the Manchester Regional Hospital Board. Leaving much non-medical University work to Professor (now Sir William) Pugh, as Deputy Vice-Chancellor, and leaving to that mild but determined man Henry Stanley Raper the task of working out the implications of the new regime as affecting the University and the Teaching Hospital (though in both cases retaining a mastery of affairs, remarkable for its detail) Stopford, with Dr F. N. Marshall as his henchman, set about the task of re-forming the hospital system within his region. The energy and concentration he put into the task was little short of phenomenal. The friends, retired as well as active, with whom he had worked in clinical medicine over the years found themselves impressed ('shanghaied', one of them describes it) into various kinds of voluntary and advisory jobs. The medical and ancillary services responded to a leadership which had something impassioned about it, and the lay members of the Board and of Management Committees, some of whom were still resentful at the change, nevertheless yielded to the patent sincerity and friendliness of their Chairman. It was a remarkable Indian Summer for in 1948 Stopford was already a man of sixty who was at war with his own medical advisers who were unanimous in advising less work and he being ruthless in accepting more. In 1953 Stopford's date of retirement as Vice-Chancellor approached but the University sought to persuade him to accept a further three years' appointment, laying down the one condition that he and his wife should, before entering on the final spell, take a holiday. They spent this holiday by travelling to New Zealand and Australia by way of the Panama Canal, returning by Ceylon and India. It was a mistake. Events showed that the wiser course would have been to let him have the holiday when the main task was over. The strain of taking up the reins again was almost crushing, but despite much sickness (often unsuspected by those working with him) he remained completely absorbed in his tasks, choosing the occasion however to retire from the Chairmanship of the Regional Board.

He received many honours. In 1920 he was appointed M.B.E., presumably in recognition of his work amongst the wounded of the 1914-1918 war; K.B.E. in 1955. He was knighted in 1941. In 1958 his name appeared amongst the first list of life peers. He held honorary degrees, in Science of the Universities of Cambridge, Dublin and Leeds, in Law of the Universities of Durham, Liverpool and Manchester. He was also awarded the Honorary Fellowships of the Royal College of Physicians and of the Royal College of Surgeons.

His retirement from the University evoked a series of demonstrations unusual even for one whose life had been so uniquely spent in the service of one institution as had been Stopford's. Chief among the distinctions falling to him and which, thereafter was to hold a very special place in his regard, was his appointment as an Honorary Freeman of the City of Manchester, and his election as an Honorary Associate of the Manchester College of Science and Technology in the translation of which from a local to a national institution he had played, along with the Earl of Woolton, a decisive role.

After his retirement, though his friends were increasingly concerned about the strain to which he was subjecting himself, he kept up many interests and fulfilled many duties. His work for the Nuffield Foundation continued unabated. He had been appointed Vice-Chairman from the start in 1943 until his death and his contribution to its development has been described as 'immense', he had considerable influence in the formation of its policy and his judgement helped his fellow trustees to a discriminating choice of the projects to be advanced. With two of the activities of the Foundation he was particularly identified. The first was the Foundation's Rheumatism Committee set up in 1948 to advise the Trustees on the use to be made of the funds at their disposal for research into rheumatism, of which Stopford was chairman from the beginning. The other enterprise, from which he extracted much joy, was his chairmanship of the selection committee for Nuffield Medical Fellowships, which was one of the first of the Foundation's fellowship schemes and which, under his guidance, became something of a prototype. Another activity which continued after retirement was his association with the Ministry of Health. For many years his advice had been sought by Chief Medical Officers and others on health service problems. After his retirement he accepted the chairmanship of the National Selection Committee of the scheme for the recruitment of hospital administrators on a national basis and he continued in this work until April 1960. Testimony is unanimous that much of the success of the scheme derives from his skill in launching it and in guiding the selection of candidates.

Nor did retirement mean severance from his University. In the later years of his Vice-Chancellorship he had launched with the Earl of Woolton an appeal for the provision of Halls of Residence in the University. Together they raised £800000. He remained the chairman of the committee responsible for the first two projects, until Woolton Hall and Needham Hall were built and he remained, until the end, a member of the University Court. Like so many others, the days of retirement were filled with duties, and in addition to those already outlined he represented the University on the Westmorland Education Committee and until just before his death was a most active member of the Lancaster and Kendal Hospital Management Committee.

Stopford's was a life lived at full intensity with a working load which may have bent but never broke him. His zest, the intensity of his interests remained to the end. In its simplicity and directness his life had a singularity which few attain and the joy in simple things, his love of football, the hours he spent at the University Athletic Ground, his interest in his garden were an abiding refreshment to him.

W. E. Le Gros Clark
W. Mansfield Cooper

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F. J. M. Stratton

FREDERICK JOHN MARRIAN STRATTON

1881-1960

Few Fellows of our Society of recent years have had such varied activities as the subject of this memoir, who died on 2 September 1960 after a full and busy life. College and University officer and administrator, astronomer, soldier, Unitarian, prominent in affairs of international science, in psychical research, in the public life of the city of Cambridge and of the county—in all these different ways he served with great distinction. To some he was known as Professor Stratton, to many in the Services and in public life as Colonel Stratton, but to a multitude of friends and acquaintances he was just 'Chubby'. This name, bestowed upon him in his early days in College to describe his physical appearance—he was short and solidly built, with a round, pleasant face—became to many as much a mark of their affection as a nickname. For he was much loved. I have known no other man who had so many friends and so many acquaintances anxious to become his friend. Among many outstanding traits of character he had a genius for making friends wherever he went, friends of all ages and of all kinds. And, in the midst of his very busy life, he managed somehow or other to keep his friendships in constant repair.

Liberal-minded, rational, generous with his time and with his money—though his means were never more than modest—he was always ready to undertake any task or responsibility which his conscience and his sense of duty recommended to him. He was completely unselfish, and his own convenience and comfort counted not at all when he could serve a friend or a cause to which he belonged. Sociable and hospitable, he radiated goodwill, and his zest for life raised the tempo of it for others; it was easy to understand why so many were glad to know him and to welcome his company. But he knew others better than they knew him, for it was not easy to know him really well. He spoke little about himself or his doings; it was not only that he was the most modest and unassuming of men, he was very reticent about his personal affairs and he seldom divulged his inner thoughts even to those nearest to him. He set up no barrier; he was just self-contained, at peace with himself.

Moreover, he tended to keep the different aspects of his life in separate compartments. There was, so to say, Stratton in College, Stratton the soldier, Stratton the Unitarian, Stratton of the British Legion, and so on. All these activities met in him, but rarely did he allow them to overlap and to mingle. Many who knew him well in one sphere might have but the vaguest idea, if that, of his activity in another. In his latter years, when I was Master of Caius, I was perhaps as close to him as anyone; he would never let more than a week or so go by without dropping in for a cup of tea before he went to bed. I knew something about all his activities, but much of what I knew came from friends we happened to have in common; unless it concerned me, he himself seldom made more than a casual mention of what he had been doing, and then usually to explain some temporary absence or to give me news of a friend. I cannot remember a single instance of his claiming credit for anything he had done, or even implying it.

So it came about that very few appreciated what a remarkable man Stratton really was, or realized how much he had achieved in so many and such different spheres of work. I believe that he did not wish otherwise.

He had of course certain advantages. He had a robust constitution and enviable powers of digestion. He suffered from serious illness only three times in his life before the last fatal illness, and only one of these was in his busy period; and his work was seldom, if ever, interrupted by the minor complaints to which most of us fall prey; he always seemed to be in full vigour. Until his later years, he needed—or could do with—only four hours or so sleep a night for weeks at a time. A bachelor living in College, he had no domestic worries or duties—or distractions. Thus he had more time for work and play in the twenty-four hours of a day than most of us can find. He made use of his time. As Lord McNair has said: 'At the same moment he could be Senior Tutor, University Lecturer, a member of the Council of the Senate, preparing an eclipse expedition and spending one day a week in London as an officer of the Royal Astronomical Society.' Nor was this all; one could add his work for the Unitarian church, in psychical research, on behalf of many causes in town and country—all going smoothly at the same time. Clearly he had an immense capacity for work; he had also a gift for effective, but unobtrusive, organization and the ability to get things done in the right way with the least use of words, whether written or spoken. He was good at choosing men for their tasks, clear and explicit in defining their limits of responsibility; and then he would let them get on with the job. He had a quick, decisive mind with a sure grasp of essentials, and this quality gained him the confidence of his fellow-workers. But, in addition, he set an example of unselfishness, of devotion and loyalty which inspired others to emulate him; by giving the best of himself he got the best from them. It was his character as much as his mental equipment which made him so successful an organizer.

He organized himself as efficiently as he organized others. He knew his powers and worked within his limits. He never strove for perfection or even hankered after it. While seeing the ideal solution to a problem which he had to deal with, he realized that time was a vital factor and that a good solution quickly applied was nearly always more effective than the best solution if delayed. This—sound judgment quickly followed by clear decision—was, I believe, the secret of his ability to do so many things so well at the same time.

In his prime he was always too busy but always in quiet control, never flustered or fussy, never fretful and never a sign of 'nerves' or ill temper; and never too busy to find time for the company of his friends. Age, which so often erodes character, revealing and exaggerating any defects, made little change in him; he continued as generous, as serene and equable as ever, he became merely a little slower in body and in mind.

Stratton was indeed a man of great quality; his many gifts were used in the service of his fellow-men, with no thought for personal profit of any kind. He has left happy, unclouded memories with all who knew him, for he was a most genuine and lovable man. There was not a particle of base metal in him.

Early life and academic career

Frederick John Marrian Stratton was born at Edgbaston, Birmingham, on 16 October 1881, the youngest of a family of six sons and two daughters. His father was Stephen Samuel Stratton, musical critic and historian, and his mother Mary Jane Marrian. His childhood memories were naturally of various musical activities in his home and in the city of Birmingham—concerts, musical festivals and the like; among the many musicans who visited his father, he remembered Ebenezer Prout and Dvořák.

In 1891 he went, with an entrance scholarship, to King Edward's Grammar School, Five Ways, Birmingham, of which the Headmaster was the Rev. E. F. McCarthy. In 1897 he proceeded to Mason University College, as an arts student working for London Matriculation and the Intermediate Arts Examination. It was here that he began to develop his interest in mathematics, under the teaching of Professor R. S. Heath and Mr H. W. Austin. In his last year there Stratton, as a member of the Student Representative Council, came into contact with Oliver Lodge, who had been appointed Principal of the newly formed University—a contact, which, though perhaps never close, was maintained until Lodge's death, through their common interest in psychical research as well as in physical science and astronomy.

In 1900 Stratton won, by examination, an Entrance Scholarship in Mathematics to Gonville and Caius College, Cambridge. He went into residence in October 1901 and, in effect, he was in residence in College and Cambridge for the rest of his life.

His academic career was soon marked by several distinctions. After taking a London B.A. (External) in 1903—in Greek, Latin, and mathematics—he graduated as Third Wrangler in Part I of the Mathematical Tripos in 1904, the year in which Eddington was Senior Wrangler. In the following year he was placed in Class 1, Division 2 in Part II of the Tripos, and awarded the Tyson Medal in astronomy. He was elected to an Isaac Newton Studentship in the same year. In 1906 he won a Smith's Prize. Also in 1906 he was elected to a Fellowship of his College, and he remained a Fellow until his death.

He began his researches in astronomy in 1905. His early investigations ranged rather widely, and he wrote papers on planetary motions and on the latitude drift of sunspots. However, the appearance of Nova Geminorum in 1912 excited his imagination, and the study of novae, particularly the changes in their spectra and the correlation of these with magnitude changes, became his special interest for the rest of his scientific life. He had already in 1909 been appointed Assistant Director in Astrophysics at the University Observatory, and in 1913, largely as a result of his work on Nova Geminorum, he was chosen by Professor H. F. Newall as Assistant Director of the Solar Physics Observatory, being appointed at the same time to a University Lectureship in astrophysics.

This work was broken off by the First World War, during the whole of which Stratton was in the Army; then, on his return to Cambridge, he became a Tutor of his College, and in 1921 Senior Tutor. This was at a very lively but difficult time when the University and the Colleges were in a very fluid state and when the duties of Senior Tutor were very demanding. Stratton relinquished his official connexion with the Solar Physics Observatory, but with his efficient organization of his day—and night—he managed to maintain his astronomical interests. He continued to give lectures on astronomy, among them one of the first general courses on astrophysics in this country; and he published a book on *Astronomical physics* in 1925 which remained for many years one of the most useful short treatises in this field. It was described by Walter Adams of the Mount Wilson Observatory as 'one of the finest relatively short treatises in its field, and still a reference book for astronomers and laymen, in spite of the quarter of a century since it appeared'. He was able to continue some work on novae, making observations of the later stages of Nova Aquilae 1918 and on Nova Cygni 1920. He also contributed a scholarly article on novae to Volume VI of the *Handbuch der Astrophysik* published in 1928. A second article was published in 1934 in Volume VII. He was Halley Lecturer at Oxford in 1927.

When H. F. Newall retired in 1928 Stratton was the obvious successor to the Chair of Astrophysics, which carried with it the Directorship of the Solar Physics Laboratory. He then had to resign his College offices—the Senior Tutorship and the College Lectureship which he had held since 1910—and he was able to give the greater part of his energy to his new duties and to his own interests in astrophysics. He held the Chair until 1947 but his tenure was interrupted by an illness in 1931, which kept him out of action for some months, and later by the Second World War, after which there was little time left for scientific work.

His chief interest in astrophysics remained in the spectrum changes of new stars, and he wrote memoirs on Nova Persei 1901 and Nova Herculis 1934. The outburst of Nova Herculis in 1934 came at a fortunate time, when his health had been restored to its former vigour. Stratton and his colleagues in the Solar Physics Observatory, notwithstanding the rather meagre equipment at their disposal, were able to obtain a remarkable record of the spectrum changes of this very interesting star, as it proved to be. His studies of this spectrum, its changes and their correlation with the magnitude changes of the nova, kept him busy for several years and resulted in his most important publications in astrophysics. The work on Nova Herculis culminated in the production, with W. H. Manning, of the *Atlas of the spectrum of Nova Herculis*, in which they examined and collated evidence from all over the world. Professor Redman has said 'This is still one of the most complete records we have of a nova outburst.'

The work on Nova Herculis was not only the most important of his contributions to astrophysics, it was also the last, for the Second World War took him away from Cambridge and the Observatory, and on its conclusion he had only two years to go before reaching the retiring age. In the conditions of that time there was little opportunity to instigate new work, and his main scientific interest became the completion of a useful history of the Cambridge Observatories up to 1947.

One of the greatest pleasures which Stratton enjoyed by virtue of his position as Professor and Director of the Solar Physics Observatory was the opportunity to help and to encourage younger men working in his field. Among his collaborators and colleagues of this period may be mentioned R. O. Redman, who succeeded him in 1947, R. v.d. R. Woolley, now Astronomer Royal, R. H. Stoy, Astronomer at the Cape of Good Hope, and Mrs Cecilia Payne-Gaposchkin.

A short account must be given of Stratton's work on solar eclipses, short because he was pursued by bad luck on almost every occasion. As early as 1914 he went with Newall to the Crimea intending to observe the total eclipse of 21 August, but on the outbreak of the First War he hurried back to England to join the Army. In 1923 he became secretary of the Joint Permanent Eclipse Committee of the Royal Society and the Royal Astronomical Society, and he organized and directed four eclipse expeditions—to Sumatra (1926), Siam (1929), Canada (1932) and Japan (1936). Only at Sumatra did he have fine weather. There, with C. R. Davidson, he made successful observations of the chromospheric spectrum. The Siam and Canada expeditions, the latter quite a large party, were almost completely marred by bad weather and failed to give results of any value. In Japan, where he was accompanied by R. O. Redman, R. A. Bagnold, T. Royds and C. W. Allen, he was more fortunate, but even on this occasion success was only partial; the sun was covered by a cloud almost at the instant of second contact. However, some good spectroscopic observations were made which proved very useful to other workers in later years.

To this catalogue of misfortune one other occasion may be added—the eclipse in Norway in 1927 to which Newall took a party, including Stratton, from the Solar Physics Observatory. This also was a complete failure.

In addition to his work on novae and on solar eclipses Stratton took an active and prominent part for many years in the organization of science both in this country and internationally.

He was editor of *The Observatory* from 1913 to 1925. He was of course closely associated with the Royal Astronomical Society, serving as Treasurer from

1923-1927, as President 1933-1935, and as Foreign Secretary from 1945-1955. He was one of the Honorary Secretaries of the British Association for the Advancement of Science in 1929, and General Secretary from 1930 to 1935.

The more interesting side of his activities of this nature was in the international field, for here especially his influence went far beyond the formal duties of the offices he held and his faculty of creating good fellowship around him proved of inestimable value. He did much to promote the interests of the International Astronomical Union, one of the earliest international scientific unions, and was its General Secretary from 1925 to 1935. He travelled widely on its affairs, making many friends among astronomers of all countries. He edited the *Proceedings of the I.A.U.* in the three volumes which reported the meetings of the Union in Leiden, Cambridge (Mass.) and Paris. He also served as chairman of the Commissions of the I.A.U. on Eclipses, Exchange of astronomers between countries, and on Novae.

A post of considerable importance was that of General Secretary of the International Council of Scientific Unions which he held from 1937 to 1952, when advancing age and a serious illness convinced him that he must begin to restrict his activities, especially those which involved much travelling.

The high regard in which he was widely held was shown in marked manner on the approach of his seventieth birthday. To celebrate this event some of his pupils undertook, with Dr A. Beer as editor, to produce what is generally called a Festschrift. But as more and more of his friends came to hear of it, they asked to take part; and what was to have been a modest book expanded into *Vistas in astronomy* in two thick volumes—and consequently late in appearing. Nor was this the end, for a third volume was produced, and others are still to come.

This gave Stratton enormous pleasure, coupled with some amusement at the growth of the Festschrift into these weighty and formidable volumes. I well remember the occasion on which he showed me the first volume, and his great delight in seeing the fly-leaves covered with the signatures of nearly every astronomer in the world, about each of whom he had something kind to remember and to say. He also showed me, in one of the volumes, a photograph of some part of the sky which bore a small white dot, aptly named Strattonia; for it was quite small and perfectly spherical.

Gonville and Caius College

From the day he entered Caius in October 1901 Stratton took a large part in the life of the College in almost all its aspects, and this association grew ever more and more intimate. He was continuously in residence, except for the periods on war service and for short voluntary or involuntary absences, from October 1901 until a few weeks before his death, and for some fifty years Caius was his home and the centre of his life.

He was on the Foundation for nearly 60 years, entering as a Scholar, being elected Fellow in 1906, and so remaining until his death. For some years at the end he was the senior Fellow resident in Cambridge. He held many College offices—Steward 1910-1912, College Lecturer 1910-1929, Tutor 1919-1921 and Senior Tutor 1921-1928, and President 1946-1948.

By far the most important of these offices was that of Tutor, including of course Senior Tutor. He became Tutor at a critical time, when large numbers of men came to Cambridge after the First World War, many having been in the Services, and when University and Colleges were in a state of upheaval, faced with difficult re-adjustments and some quite radical changes. He was fortunate in having colleagues of outstanding ability and character, and in that the Master of the College at that time, H. K. Anderson, was a man of quite exceptional quality, a great initiator and reformer in the College and in the University. But undoubtedly some considerable share in the high reputation which Caius made in those years must be given to Stratton and his influence on the undergraduates. His understanding of the ways of young men was re-inforced by his experience of command in the Army. While far from repressing their high spirits he maintained a sound discipline; and his own devotion to the College inspired them—or most of them—with a strong sense of loyalty and a sense of responsibility. He took a benevolent interest in all the legitimate activities of his pupils, and thoroughly enjoyed any success they achieved, whether academic, in games or in other pursuits. Many became his firm friends and remained so long after they had gone down.

When, in 1928, he had to relinquish the office of Senior Tutor his contacts with undergraduates inevitably diminished, but it was surprising how many he still made time and opportunity to know. In his later years, many sons and close relatives of Caians he had known entered the College; he always invited them to his rooms so that he could make their acquaintance and he would follow their progress and would be ready to help in any way open to him.

No other Caian of these times knew so many members of the College, no one was so diligent in keeping in touch with Caians or in seeking them out wherever his travels took him. His rooms in College were the first port of call for most members of the College visiting Cambridge, and they could always be sure of a warm welcome and a friendly talk. His prodigious memory served him well on these as on other occasions, for he would mention quite naturally all manner of details about their undergraduate days, and, in many cases, it would soon become apparent that he had followed their subsequent careers.

During my years as Master of Caius I naturally relied a good deal on his long and intimate knowledge of College affairs. His counsel was always generously given, and in a most considerate way; it was never thrust at me. He was always ready to do what he could, however humdrum, for the benefit of the College or its members. It was typical of him, when well over seventy, to assume responsibility for the *College Biographical History*—and to get two new volumes published.

Few men have served their College with such devotion and loyalty as Chubby; and, in return, few men have received such affection and respect.

Military and War Service

Stratton had a long and distinguished association with the Army and he took great pride and pleasure in it; he was always happy in the company of soldiers.

His military activities began not later than 1901, for on going up to Cambridge he joined 'C' Company (the Caius Company) of the C.U. Rifle Volunteers, which became in 1908 the C.U. Officers Training Corps. He was at least partly instrumental in forming the Communication Company, later known as Signals, and he commanded the Signal Unit from 1910 to 1928.

When the First World War broke out he was an obvious choice to establish and command one of the many new Divisional Signal Companies which were needed for the transition from the old conception of Signals to a new and highly technical auxiliary in warfare. He became O.C. Signals of the 20th Divisional Signal Company, R.E. He took his Company to France in the summer of 1915 to join the B.E.F., where it formed part of the 14th Corps. This Corps had a prominent share in the prolonged and bloody Battle of the Somme in 1916. In 1917 he was promoted Lieutenant-Colonel and became Assistant Director Signals (later designated C.S.O.) of the newly formed 19th Corps B.E.F. under the command of Lt.-General Watts. This Corps was involved in the catastrophic retreat of the 5th Army in March 1918, when Stratton showed outstanding ability in keeping his signal communications working satisfactorily. A fellow officer of those days, now Brigadier R. A. Bagnold, F.R.S., has written:

'At one time Stratton was keeping Line or Despatch Rider communication going with 17 separate divisional headquarters, each moving back once or twice a day, usually at night, without warning . . . He was an outstanding personality. After the G.O.C. and G.S.O. 1. "Stratton the astronomer" was probably the best-known man in the 20th Division. His little chubby figure, his puckish wit and radiating friendliness were welcomed everywhere. He combined a masterful grasp of the general situation with an extraordinary knack of managing people and getting the best out of them. He had the reputation of never needing sleep (his average was not more than three hours a day). And in March 1918 he was still cheerful and still had the whole situation in hand after five days without any sleep at all.'

Brigadier Bagnold has also recorded a remark by his Corps Commander—'I can't understand that chap. If he'd been a regular instead of an astronomer he'd have commanded Gough's army better than anyone. How does he do it?'

Stratton was mentioned five times in despatches, and he was awarded the D.S.O. and the Croix de Chevalier, Légion d'Honneur.

By the end of the war his influence in the Royal Corps of Signals must have been considerable. He was much trusted, and indeed beloved, by General Watts and all the Staff of 19th Corps Headquarters; to them he was 'Don'. He seems to have enjoyed every minute of his military life and, in his unspoilt, unassuming way, the company of his Generals.

On his return to Cambridge in 1919, he re-formed the Signal section of the C.U.O.T.C., which he commanded until 1928 and with which he remained in contact until his death. He was active in forming the British Legion in Cambridgeshire and the Isle of Ely, and he was County Chairman for many years. (I have been told that he was largely responsible for instigating the undergraduates' methods of raising money on Poppy Day—I can well believe it.) He was active also with the Cambridgeshire Territorial Army Association, becoming the University representative in 1921 and Chairman from 1934-1940. He took every opportunity of renewing contacts with the Army and especially with the Corps of Signals. The regular officers who came to Cambridge to supplement their technical training found in him a host and friend who took a warm interest in all their doings.

When the Second World War began Stratton was close on 58 and, in the opinion of the War Office, too old for the active employment he so ardently desired. But nothing could long withstand his persistence and ingenuity, and his determination to serve in some way or other. His first task was to help to run an O.C.T.U., but after a short time the War Office directed him to a more suitable post in which his scientific knowledge and his friendly common sense found better employment. This service consisted of Special Duties connected with radio security, and it required him to travel widely—to Canada, U.S.A., Australia, India, Ceylon, New Zealand, New Guinea, New Hebrides and Fiji—on missions whose nature has not yet been disclosed to me. His own account of these missions did not go beyond mentioning his meetings with mutual friends and with members of the College residing or on duty in these countries, but he could not disguise his great satisfaction in being again in the Army and in uniform, and in serving his country even in this unmentionable capacity. His rank was that of Brevet Lt.-Colonel in the Royal Corps of Signals.

This service ended in 1945 and he returned to Cambridge to resume his post as Professor of Astrophysics, with only two years to go before retirement. He was soon invited by the War Office to give his services yet again to Army problems. The War Office was trying to retain, in peace, some of the close affiliations with the scientific world which it had built up during the war and which it cherished. Thus he served as Deputy Scientific Adviser to the Army Council for two years from 1948-1950, and in this capacity his wide acquaintance with the scientific world, his experience of international conferences and his appreciative understanding of Army problems were of the greatest value in ensuring progress and in promoting good relations within his sphere of activity.

He had become a Deputy Lieutenant of the county of Cambridgeshire in 1924 and, on several occasions after 1945, he acted as Vice-Lieutenant during the absences abroad of the Lord Lieutenant. He discharged these duties with his customary aplomb and thoroughly enjoyed doing so, especially those with a military aspect.

Stratton the Unitarian

One of Stratton's deepest interests, but one which he seldom even mentioned except to those of his own persuasion, was the cause of liberal religion and the system of Christian thought and religious observance held by the Unitarian church.

His parents were members of the Church of the Messiah, Birmingham, and he was brought up in that congregation. The Minister at that time was the Reverend L. P. Jacks, who later became the Principal of Manchester College, Oxford and Editor of *The Hibbert Journal*. He was a man of considerable personality, and Stratton himself has recorded that Dr Jacks had a strong influence on his outlook on life.

When, in 1906, Stratton was elected a Fellow of his College, and saw that he was settling down in Cambridge, he gathered round him a group of interested people and founded the Unitarian cause in Cambridge. For the first four years of its existence he was secretary of this group. He became Chairman of the Congregation in 1910 and he occupied this office until his death. During this period the Memorial Church was built in Emmanuel Road; it was opened in 1928. The affairs and especially the people of the Memorial Church were very close to his heart.

He will long be remembered for his services to the Unitarian denomination and its causes not only in Cambridge but throughout the country. He served on most of the important committees, he gave the Essex Hall Lecture in 1947, and he was President of the General Assembly of Unitarian and Free Christian Churches in 1948-1949. He was appointed to the Hibbert Trust in 1925 and was Chairman of the Board of Trustees from 1948. For many years he was a member of the Council of Manchester College, Oxford (a theological training college for those entering the Unitarian ministry) and Chairman of the Council from 1949-1952; in 1949-1951 he was President of the College.

Stratton made a profound impression in the congregation of Unitarians in Cambridge. The Rev H. Stewart Carter, Minister of the Memorial Church, to whom I owe most of this section, writes: 'Never too busy to undertake tasks and responsibilities pertaining to the enrichment of man's moral and spiritual nature, and to help those in need, there is a great multitude of men who are now serving God and their generation, who have cause to remember the encouragement and the friendship he unstintedly gave them in their undergraduate days and after.'

I have already said that Stratton seldom mentioned his activity and deep interest in religious matters and the Unitarian cause. I knew something about this aspect of his life from Unitarian friends; he knew this but nevertheless seldom said a word about it. It was typical of his reticence that, in some notes he made for the Royal Society about his activities, the only reference to this side of his life is 'Hibbert Trustee 1925—'.

Psychical research

Dr E. J. Dingwall writes:

'Stratton joined the Society for Psychical Research in 1902, when he had come up to Cambridge with an introduction from Sir Oliver Lodge to Mrs Sidgwick. It was shortly after the death of Professor Henry Sidgwick that young Stratton used to be invited round by Mrs Sidgwick to have tea with her and to meet her brothers Arthur and Gerald Balfour, who were later to take such an important part in the series of experiments called "the cross-correspondences".

'During his many years of membership of the Society he met all the prominent members who were active within that organization, and his training as a scientist led him to take more than passing interest in the physical phenomena of spiritualism rather than in that section devoted to automatisms. In this interest he was encouraged by the indefatigable and brilliant Everard Feilding and by the energetic Ernest Bennett who managed to give a good deal of time to psychical research in spite of his increasing political interests.

'In dealing with the physical phenomena Stratton, like so many other critical investigators, found it difficult to investigate the phenomena in the séance room, since his busy life prevented him from giving the time systematically to explore these elusive phenomena. His interests therefore turned towards inquiries into haunted houses and poltergeists, few of which involved the waste of time incurred in investigating physical mediums, and which did not necessitate the knowledge of trickery. Among these his investigation of the famous haunted house in Cambridge where J. C. Lawson of Pembroke College lived for some time occupied his attention for many years and remained a subject for discussion until the day of his death.

'From 1953-1955 he occupied the Presidential chair in the Society for Psychical Research, a period which was one of the most chequered in the Society's history owing mainly to the strong controversy aroused by the report on the haunting of Borley Rectory.

'During the latter years of his life Stratton's interest in haunted houses was increased by the revival of the underground water theory as an explanation for mysterious noises; and he was always keen to be informed of any recent developments in this direction, especially as an indubitable case of this nature had been discovered and investigated in Yorkshire.'

There is little that I can add of my own knowledge, although he often spoke to me of his interest in psychical phenomena. He looked at these things rationally. He seemed to be always ready to believe that there were some phenomena which could not be fully explained on a physical basis but there was never enough evidence to convince him. In his last years, when he had some free time, he investigated several haunted houses in the neighbourhood of Cambridge, using modern instruments to record and identify the reported 'noises'; but the 'noises' did not oblige. There was also a case of telepathy—perhaps not quite the correct description for this case—which was reported to him by a medical friend and which excited his lively interest. With Sir Rudolph Peters, he gave many hours of close investigation to it; unfortunately, the results were not decisive one way or the other.

Stratton was elected to the Royal Society in 1947.

He was a member of many learned societies in this country and abroad. Some of these have been mentioned; it would be tedious to record them all.

In preparing this memoir I have had the assistance, most generously and freely given, of many of Stratton's friends and colleagues. To the names of the Reverend H. Stewart Carter and Dr E. J. Dingwall, from whom I have quoted in the text, I wish to add those of Sir Owen Wansbrough-Jones and Mr Duncan Norman, who supplied me with information about Army service; to them, and to the many others who helped me but whom I do not name here, I am deeply grateful.

James Chadwick

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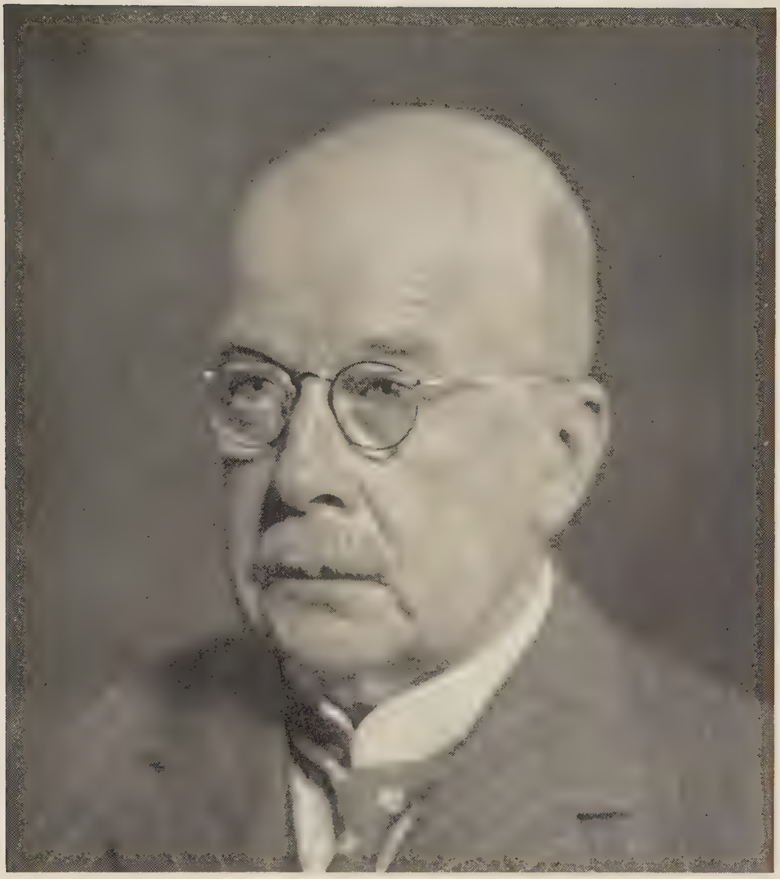

Nils C.

NILS EBERHARD SVEDELIUS

1873-1960

Nils Svedelius came of old Swedish stock. His first known ancestor was one Nils Andersson, a farmer in the parish of Leksand in Dalecarlia, who lived in the middle of the 16th century. In the early 17th century one of his grandsons entered holy orders and became vicar of Rättvik parish. In those days, small-scale farming was an important side activity for country parsons. During his tenure, a good piece of land belonging to the parsonage and known as Sveden was brought under the plough, and from this place his grandchildren took the family name Svedelius. From them all the bearers of the name are descended, among them many prominent citizens, high government officials, army officers, merchants and teachers, the most widely known being the historian Vilhelm Erik Svedelius (1816-1889), still something of a legendary figure in the academic annals of Uppsala. But as far as is known to the present writer, none of them ever took any special interest in natural history. Only one of them, the man whose life and work are the object of this article, became a man of science.

Nils Eberhard Svedelius was born in Stockholm on 5 August 1873, the second son of Supreme Court Justice Carl Svedelius, L.L.D., and of his wife Ebba Katarina, who came of the old noble family Skytte af Sätra. Nils's elder brother studied law and, like his father, became a Justice of the Supreme Court of Sweden. Even as a boy, Nils showed a keen interest in nature. One of his teachers at school was a good botanist and knew how to stimulate his pupils, especially by arranging excursions to various parts of Sweden. In his autobiographical notes, deposited with the Swedish Academy of Science, Svedelius praises him as the person responsible for his decision to devote his life to botany. After matriculation in 1891 he entered the University of Uppsala. The two professors of botany there were Thore Magnus Fries (1832-1913), head of the department, a prominent lichenologist and widely known as the author of standard works on the life and letters of Linnaeus; and Frans Reinhold Kjellman (1846-1907), who succeeded Fries in 1899. Kjellman's main field was marine phycology, but he was also an accomplished morphologist; students of his will remember his original life-form system. Kjellman made his mark in the Arctic; he had wintered in Spitzbergen and went as botanist with A. E. Nordenskiöld on board the *Vega*, the first ship to complete the North-East Passage. Kjellman's publications on Arctic plant life belong to the classics. His *The algae of the Arctic sea* is a fundamental work. Many Swedish botanists owed their education to him. Of the pupils he himself considered most promising, two were to join the ranks of the foremost algologists of our time. They were Harald Kylin (1879-1949) and Nils Svedelius. Alas, Kjellman did not live to witness their success. It fell to Svedelius to write the biography of his friend and teacher.

In 1895 Svedelius acquired his first university degree, that of candidate of philosophy. His subjects, beside botany, where zoology, chemistry, mathematics and theoretical philosophy. During the following years he specialized in phycology, made long excursions along the coasts of Scandinavia and worked at our marine biological station in Bohuslän. In the past, interest in marine life had focused on our North Sea coast with its higher salinity and much richer flora, while the Baltic had been rather neglected. With the approval of Kjellman, Svedelius selected the southern section of our east coast as his working field. The low salinity there, decreasing as one advances northwards, still permits the existence of a limited number of species, in many cases represented by reduced forms which offered various ecological and morphological problems. In March 1900 Svedelius graduated as licentiate of philosophy, with chemistry as his second subject, and in May 1901 he defended his doctor's thesis, 'Studier öfver Östersjöns hafsalgflora' (Studies in the marine algae of the Baltic Sea), and won the degree of doctor of philosophy. Although his field work had been limited to the east coast of Småland and the island of Gotland, and the number of species reported did not exceed 50, his general chapters on the external factors, the ecology of the vertical belts and the composition of the communities, as well as on the history of the flora, were good enough to earn the high marks required for the position of *docent*, and he received the appointment as such in October 1902. In the meantime he filled the post of Regnellian amanuensis at the Natural History Museum (Riksmuseum) in Stockholm, where his duties were limited to the taxonomy of vascular plants belonging to the collections from tropical South America. From his school days he had retained an interest in higher plants. As early as in the nineties he published two papers on the Juncaceae, one of them a list of the species collected by the first Regnellian expedition to Brazil, and as amanuensis he completed a study on saprophytic Gentianaceae. I do not think that he ever had the intention of continuing along these lines. Phycology remained his main interest, and it was not long before he got an opportunity to show his mettle. In those days a Swedish botanist could consider himself lucky if he got a chance to visit Lapland or the mountains of Norway. We used to envy our Danish neighbours who could secure a cheap passage to their West Indian colony. But of the few large travelling scholarships available to us, one fell to the highly qualified *docent* Svedelius and enabled him to spend the greater part of one year (1902-3) in the Far East. He concentrated on Ceylon, but also visited Singapore and Java, working at Buitenzorg under Melchior Treub. His main object was to survey the marine algae of Ceylon and study their ecology and distribution in detail, this to be followed by a systematic study of the large collections he brought back with him. His plan was to present the results in a series of papers entitled 'Reports on the marine algae of Ceylon', and in 1906 No. 1 appeared. Under the title 'Ecological and systematic studies of the Ceylon species of Caulerpa', it described 21 species, two of them new to science. The same year he published a most interesting report on the large coral reef around Galle, the fruit of a full eight months of field work. The most important result of this survey was that he established the connexion between the periodicity of the algae and the change of monsoons.

The 'Reports' were, however, discontinued; a No. 2 never appeared, but we have little reason to regret this. Other algologists, among whom Mrs Weber van Bosse deserves to be mentioned in first place, have specialized in the marine flora of the Indian Ocean and greatly enriched our knowledge of it. Svedelius did not neglect systematic and taxonomic questions when they called for attention, but he became too deeply interested in the life history of a number of outstanding types to spend his time in a classification of his Ceylon collection.

Kjellman belonged to the old school, which confined research in algology to an investigation of the external morphology and gross anatomy by means of hand sections with a razor. In his institute there was nothing one could call a laboratory, no microscope with high-power lenses, no microtome. Cytological technique came to Uppsala with Oscar Juel (1863-1913) *docent* in botany since 1892. Juel became famous for his discovery of the first case of parthenogenesis in flowering plants (*Antennaria alpina*, 1898), barely before the second case (*Alchemilla vulgaris*) was found by Svante Murbeck (1859-1944), professor at Lund. The papers of both were published in 1900. In 1907 Juel succeeded Kjellman as professor and director of the institute, where he trained a school of cytologists. Svedelius was one of them. At Galle he had collected a species of *Martensia* (*M. gracilis*), belonging to the Delesseriaceae, where the distal part of the thallus is an elegant and complex network of cells. This structure had already been briefly described by Harvey and in more detail by J. G. Agardh, but they had arrived at quite different conclusions about the origin of the net. They had only herbarium specimens to work with, while Svedelius had a large set of specimens preserved in liquid, including both young and mature plants. Using modern technique, he was able to solve the problem and to show that both Harvey and Agardh were wrong. Including several other *Martensia* species in his study, he found that three separate morphological types could be distinguished. Of *M. gracilis* he had secured male, female and tetrasporic individuals, permitting him to give the first detailed and correct description of the development of their reproductive organs. All cells are multinucleate in *Martensia*, those of the reproductive organs not excepted, but at maturity all nuclei except for a single one degenerate. Some years later Svedelius described another instance of the same kind in *Nitophyllum*. The memoir on *Martensia* is a solid piece of work and quite characteristic of Svedelius as an author, with its completeness of detail, clarity and logical reasoning. He was also a clever draughtsman and his papers are profusely illustrated. Brevity, however, was not among his strong points. He was meticulous, and he liked to repeat and stress a statement or conclusion at the end of each chapter in the manner of a schoolmaster who spares no effort to make the facts he tells his pupils stick.

Two years before the study of *Martensia* was published, Yamanouchi's discovery of the true nature of the alternation between a haploid sexual and a diploid asexual generation in the life history of *Polysiphonia violacea* marked the beginning of a new era in the study of the red algae. Outwardly all individuals were alike, differing only in the nature of their reproductive organs, and it lay near to assume that *Martensia* behaved in the same manner, but in order to prove this properly fixed material was needed. The collection Svedelius had brought from Ceylon was preserved in formalin and was not suitable for cytological purposes. With the proper kind of material at hand, a wide field of research lay open. Did all Rhodophyceae behave like *Polysiphonia*? Of some species only sexual plants were known, of others only asexual ones. What was their secret?

Diplobionts and haplobionts, and the place of reduction in the life cycle of the Rhodophyceae

Let us take a brief look at the background of modern phycology. Alternation between a sporophyte and a gametophyte had long been recognized in ferns and bryophytes, and in 1904 J. L. Williams described the life history of a brown alga, *Dictyota dichotoma*, where, however, the two alternating generations were alike in external appearance. In the Rhodophyceae matters were complicated by the occurrence of so-called carpospores, produced by the gonimoblast as a result of fertilization. This led the illustrious German phycologist Schmitz to his opinion that the gonimoblast with its carpospores represented the true sporophyte; the tetraspore-bearing plant did not fit into the cycle, especially as it did not exist in the common and widespread *Nemalion*. Where tetraspores occurred they were a kind of by-product just as were monospores, useful for the spread of the species, but they had nothing to do with the alternation of generations. Schmitz's theory was shared by Oltmanns, who set forth his ideas on the subject in 1898 and, in more detail, in his great work *Morphologie und Biologie der Algen* (1904-1905). Strasburger himself wrote in 1906: 'Ein Grund, die Anlage der Tetrasporen der Rhodophyceen nach einer Reduktionsteiliung zu suchen, liegt ja nicht vor.' Kjellman even changed the terminology, replacing 'tetraspores' with 'tetragonidia' and 'cystocarp' with 'sporocarp', and persuaded his pupils Svedelius and Kylin to use the new terms in their dissertations. Yamanouchi's findings in *Polysiphonia*, confirmed a year later by J. F. Lewis in *Griffithsia*, entirely changed the situation. Their objects belonged to two different families, Rhodomelaceae and Ceramiaeceae, respectively. Stimulated by their success, Svedelius turned to the Delesseriaceae. *Delesseria sanguinea* was easy to obtain the year round at the Swedish west coast, and it proved to be extremely well suited to his purpose. His results were published in three separate papers, 'Über den Generationswechsel' (1911), 'Über die Spermatienbildung' (1912), and 'Über die Zyctokarpienbildung' (1914). The life cycle of *Delesseria* was exactly the same as in *Polysiphonia* and *Griffithsia*, but Svedelius knew that this was only part of the story, even if it was very likely that most Rhodophyceae developed after the same pattern. Where did reduction take place when, as in the Nemalionales, no tetraspores are found, or when they occur on individuals with normal sexual reproduction? J. J. Walker had examined *Nemalion*, but his conclusions were doubtful and later proved erroneous by Kylin (1916). The second question was answered by Svedelius (1914). He had already described the formation of normal tetrasporangia in the diploid generation ($2n = 40$) of *Nitophyllum punctatum*, where division in the tetrasporangium resulted in the formation of 4 haploid spores ($n = 20$). Supposed tetrasporangia could also occur on haploid, female individuals; they looked exactly like the normal ones and appeared to develop in the same way, but the single nucleus remaining after the dissolution of the others was found to have 20 chromosomes; reduction had fallen out, we had to do with a monospore.

Meanwhile, Svedelius had turned his attention to *Scinaia furcellata*, an exclusively sexual species belonging to the *Nemalion* assemblage, in order to find out where reduction, an indispensable sequel to fertilization, took place, and he was able to show that the first division of the fertilized nucleus is heterotypic. Only the zygote is diploid, the gonimoblast and carpospores haploid. The cystocarp is, as it were, a separate generation, a carposporophyte, which remains entirely dependent on the gametophyte; one might, in the case of most of the Rhodophyceae, where a tetraspore generation is also involved, speak of three generations. Svedelius did not recognize the carposporophyte as equal in rank to the independent generations. For the *Scinaia* type, he coined the term 'haplobionts', whereas algae with a regular alternation of tetrasporophyte and gametophyte were called 'diplobionts', terms which came into common usage (1915). Kylin found that *Nemalion* and *Batrachospermum* (Kylin 1916) agreed with *Scinaia*; possibly the same life history was common to all Nemalionales.

In 1907 Juel succeeded Kjellman as ordinarius. His own chair was left vacant for years, and during much of this period Svedelius discharged the duties pertaining to it until, in 1914, he was himself appointed Professor of Botany. Even though teaching and his membership of the faculty and the academic senate took much of his time, his scientific work was never interrupted. The life cycle of the red algae continued to engage him; as new facts came to light he summarized the actual state of our knowledge time and again, the first time in 'Das Problem des Generationswechsels' (1916). A number of papers dealing with various subjects were published during the following years. His attempts to homologize the male and female organs in the red algae deserves mention, as does his description of the development of monospores in *Helminthora*, another member of the Nemalionales, where monospores seem to be common. In the same paper he took up the question of the number of nuclei in the carpogone in *Helminthora*. He found it to be binucleate. Several cases were already known which indicated that the trichogyne had a nucleus. Kylin had come to the conclusion that this is the rule, but that the nucleus of the trichogyne soon degenerates, whereas some other authors maintained that as a rule the carpogone is uninucleate. Svedelius agreed with Kylin; the original presence of two nuclei supported his theory of a morphological male-female homology.

Sauvageau's epoch-making discovery (1916) of a microscopic gametophyte in the Laminariales stimulated Svedelius to review the entire problem again in 1918, 1921 and then in 1924, in a most entertaining paper in Swedish; in English, its title would read 'The place of the reduction division in the life cycle of plants'. There, he included all plant groups in his discussion. The localization of the meiotic division remained a paramount question to which he returned at the congresses at Ithaca and Cambridge and in his lectures at the University of London.

In 1916 Kylin was able to state that *Bonnemaisonia asparagoides* was a haplobiont, a case outside the Nemalionales. Sauvageau, who had kept in close contact with Svedelius, supplied him with material of *Asparagopsis armata*, another member of the Bonnemaisoniaceae. This one also was found to be haplobiontic; in a lecture before the annual meeting of the British Association at Leeds in 1927, Svedelius gave a preliminary report on the development, but by then he had not been able to observe where reduction took place. Other duties forced him to interrupt his investigation, but when, at last, he was able to conclude it, he not only presented a complete picture of *Asparagopsis* but also included *Bonnemaisonia* and supplemented Kylin's study in essential details (1933). Meiosis was observed in both, and both showed the life cycle of *Scinaia*. Because *Bonnemaisonia* was a haplobiont lacking tetraspores, Kylin had removed the family to Nemalionales, although he did not find the real place for reduction division. This done, it appeared more and more probable that real systematic value must be ascribed to the haplobiontic and diplobiontic course of development. Svedelius's own later discoveries made him modify his opinion. We also know now that the development in these genera seems to be more complicated than we had reason to expect. I cannot go into details here but refer to the culture experiments carried out by J. and G. Feldmann (1939, 1952).

Of many red algae only plants with tetraspores are known. This may of course be due to incomplete observation; but that cannot be the reason when we deal with widespread species where sexual individuals have been searched for diligently, but in vain. A case of this kind is *Lomentaria rosea* (Rhodymeniales), not uncommon in the sublittoral of the North Atlantic and also found on the west coast of Sweden. It had been collected in many places within its entire range, but exclusively with tetraspores, while another species, *L. clavellosa*, found with the former, had tetrasporic, male and female individuals growing together. Both form the object of one of Svedelius's most important investigations. He found that the life cycle was quite normal in the latter, where meiosis takes place at the formation of the tetraspores, whereas it is suppressed in *L. rosea*, the first case of its kind discovered. On various occasions Svedelius had expressed the opinion that the haplobionts should be regarded as more primitive than the diplobionts; for other reasons, the Nemalionales had always been regarded as primitive. A case among the much more advanced Rhodymeniales was an anomaly. Svedelius concluded that in *Lomentaria rosea* we have to do with a reduced type. His memoir was published in 1937. The results obtained made him review all the different types of development known in the Rhodophyceae (Florideernas utvecklingstyper, 1938), arranging them in a progressive series: primitive haplobionts (Nemalionales)—more highly developed haplobionts in which the gonimoblast produces tetraspores after reduction division (very few cases known, e.g. *Liagora tetrasporifera* of Børgesen)—diplobionts. *Lomentaria rosea*, a diploid asexual haplobiont with unreduced tetrasporangia, and *Ahnfeltia plicata*, a haploid asexual haplobiont with monospores, represent two different reduced types, for which the terms 'diplont' (*Lomentaria*) and 'haplont' (*Ahnfeltia*) were proposed. All the types are illustrated with very clear diagrammatic drawings.

The material on which Svedelius based the studies referred to above came from European waters, especially from marine biological stations he himself had visited, but in some instances he returned to his collection from Ceylon. In *Dermonema* (Nemalionales) he found a haplobiont with pronounced seasonal periodicity (1939). A study of *Dictyurus purpurascens* (Dasyaceae), one of the net-algae, was begun by one of his students and completed by the teacher. Both male and female organs were unknown before. The spermatangia form a very complicated and curious compound system (1946-47).

Galaxaura

It could be said that Svedelius inherited *Galaxaura* from Kjellman, and it became one of the main objects of his research work after he had retired at the age of 65 and moved to the emeritus room offered him in the new physiology building, where facilities for cytological work were excellent. In 1900 Kjellman had published a taxonomic monograph on *Galaxaura*. In some of the 62 species he recognized he found tetraspores, in others cystocarps or spermatangia, but with Oltmanns he did not believe that sexual and asexual plants represented alternating generations of one and the same species; as already mentioned, he called the tetraspores tetragonidia and the carpospores spores. He divided the genus into 7 sections, characterized by their morphology and anatomy. When Svedelius went to Ceylon, it was natural that Kjellman urged him to look for *Galaxaura*. Species of this genus were, however, quite rare at least in the intertidal belt; on the extensive reef around Galle he did not find a single species. In other localities, however, Svedelius collected six, but the material was left aside for the time being. To begin with he had no strong reason to doubt the validity of

Kjellman's opinion on the taxonomy, and when, after Kjellman's death, he took over the compilation of a supplement to the Phaeophyceae and Rhodophyceae in Engler and Prantl's *Natürliche Pflanzenfamilien*, he reproduced Kjellman's *Galaxaura* system unaltered.

In 1917-18 Marshall A. Howe published the results of his studies of the West Indian *Galaxaura ambigua*. He found that sexual and tetraspore-bearing plants always occurred together, and although he discovered that they differed from each other considerably in the cortex structure—nothing like this had ever been observed in the Rhodophyceae—he was convinced that they represented alternating generations of a single species. Kjellman had not failed to observe those differences, but from his standpoint it was natural to use them to characterize species and sections. In Howe's opinion several of Kjellman's sections were not taxonomic units at all; a 'species' referred to a sexual section might well belong, genetically, with a spore producing 'species' referred to a quite different section. It would be necessary to submit Kjellman's system to a critical revision.

It goes without saying that Howe's theory interested Svedelius tremendously. *Galaxaura* belonged to the Nemalionales, an order supposed to be strictly haplobiontic. He had shown what cytology could do, and he concluded that, in order to arrive at a reformed taxonomy, at least some species of *Galaxaura* should be subjected to a close anatomical and cytological investigation. When he found that his own Ceylon material, preserved in formalin, was unsuitable for chromosome study, he turned to Dr George Papenfuss, then at Cape Town but who was a former student of his at Uppsala. Papenfuss sent him properly fixed material of 4 species, including male, female and tetrasporic plants. Nothing was known about cytology in *Galaxaura* when Svedelius began to publish his results. Preliminary communications on the male 'conceptacles', a most uncommon structure among Florideae (1939, in Swedish), and on the cystocarps (1942) preceded his large, profusely and beautifully illustrated memoir describing the morphology, anatomy and reproduction (in German, 1942). He proved beyond doubt that, in spite of its systematic position, *Galaxaura* is a typical diplobiont with reduction division at the formation of the tetraspores. Thus haplobiontism could not be regarded as the main character in Nemalionales, even if found in the majority of genera. Kylin had claimed that in this order no auxiliary cells were present, but Svedelius argued that even if we use 'auxiliary' in the restricted sense of Kylin, *Galaxaura*, *Chaetangium*, and *Asparagopsis* have true auxiliaries. The development of the cystocarp was found to be of great interest as representing a new type. The whole carpogonial branch received diploid nuclei and became the starting point of the gonimoblast which, with the carpospores, was immersed in the thallus, protected by the calcified cortex, while no special pericarp was formed. A new classification of *Galaxaura* was attempted; instead of Kjellman's 7 sections, 3 were distinguished and the genera in Chaetangiaceae rearranged. In his monumental posthumous work on the Rhodophyceae (1956), Kylin groups them in a different way. Toward the end of his active life Svedelius turned his attention to another calcified member of the Chaetangiaceae, the monotypical *Actinotrichia*. Again Papenfuss, now living in Honolulu, supplied the material which, however, did not include the female plant, but new data were gathered concerning the development of spermatangia and tetrasporangia, and old mistakes were corrected (1952). Meanwhile Svedelius extended his work on *Galaxaura* to the taxonomy and nomenclature of the many species described. Of Kjellman's 62 species hardly more than one third stood the test, but in many cases it was impossible to arrive at a safe conclusion regarding the connexion between sporophytes and gametophytes; besides, of many species only one generation was known. In his 'Critical notes', published in 1945, Svedelius described in detail the six species he had collected around Ceylon, and in addition he cleared up many of Kjellman's species from other parts of the tropics, but he emphasized that only repeated observations in nature could indicate which sexual and tetrasporic plants belong together. Definite conclusions could, of course, be obtained only by experimental cultures at a tropical marine station, but this was a long-range project. Anyhow, close morphological and anatomical investigation could further a revision of the species. Svedelius had shown this in his 'Critical notes' and he continued along these lines in his studies on the Hawaiian species. Professor Papenfuss had sent him material of 11 species (taken in the old sense), which were subjected to a comprehensive histological analysis. In the concluding chapter, Svedelius posed the question whether or not any anatomical or morphological character could be demonstrated which would make the combination of certain sexual and asexual plants not only possible but probable. He tested the so-called 'lobed cells' and the papillae and acuminate terminal cells found in several cases and also observed by Kjellman and made use of by him in his system. The result was, however, negative; only in a single instance it appeared that two of Kjellman's species probably did belong together although he had referred them to different sections. Svedelius's last contribution to our knowledge of *Galaxaura*, a painstaking performance by an octogenarian, shows no sign of an ageing mind; the accuracy of observation and clarity of expression are unaltered, the anatomical figures drawn with the same steady hand.

In his last paper, written in August 1955 and published in 1956, Svedelius returned once more to the question whether, as he believed, primitive haplobionts had given rise to diplobionts by postponement of reduction division and the insertion of a tetraspore generation. This would signify that new gene combinations on a large scale could be expected, vastly increasing the dissociation of characters; thus, diplobionts were more advanced. In his paper on the structural evidence of genetic relationships in plants read at the Ithaca congress in 1926, Svedelius had dealt with this subject, reviewing all the facts known in the algae and comparing the course of evolution in the three large phyla. At least with regard to the Rhodophyceae, Kylin shared Svedelius's opinion. Against this the illustrious Goebel maintained that

Svedelius himself had shown that in *Scinaia* three of the four nuclei formed during meiosis degenerate, and this could not be considered a primitive character. To this Svedelius justly remarks that we know several haplobionts in which several separate gonimoblasts develop from the tetrad, and he enriched the discussion with a report on two genera of Chaetangiaceae not investigated before. One of them agreed with *Scinaia*, in the other the entire tetrad took part in gonimoblast formation. Different types were included in the family, and *Scinaia* could not be claimed as an argument against the primitiveness of haplobionts.

The geographical distribution of tropical marine algae

When preparing his doctor's thesis, Svedelius was confronted with the problem of disjunctive distribution. The flora of the Baltic was of twofold origin, including an Arctic and an Atlantic element. Several species inhabit both the Arctic Sea and the Baltic, at present separated by land. The explanation was that at one time there had been open communication between them. The tropical flora offered a great many cases of a distribution on the same pattern. The barrier need not consist of land; water with too low a temperature might have the same effect.

In his treatise on the Caulerpae of Ceylon Svedelius stated that, of the 21 species he reported on, no less than 11 also occur in the West Indies, and a similar distribution pattern characterizes species of many genera of both green, brown and red algae. In his algological work referred to above Svedelius had concentrated his research on the Florideae, although he has also published several papers not only on Chlorophyceae, but also on Phaeophyceae (see Bibliography), and in his discussion of distribution all groups were included. According to the theory of George Murray, tropical and subtropical species now occupying disjoined areas in the Atlantic and Indian-Pacific oceans, separated by the colder water around the Cape region, had been continuously distributed at an earlier epoch when the water was warmer in the area of the Cape. Svedelius did not believe that this was the reason for disjunction; former connexions across land bridges offered a much better explanation. Numerous species were found on both sides of the Isthmus of Panama, but this barrier was, geologically speaking, quite recent. Many marine phanerogams showed a similar distribution. Svedelius published his first papers dealing with these questions as early as 1906, but many years passed before he took up the problem for closer examination. Among his Ceylon algae was a species of *Neomeris* which was incompletely known; he described its peculiar structure in a paper published in 1923, and it also gave an excellent example of disjoined distribution. Many more were known in the ancient family of Dasycladaceae. The following year 'Discontinuous distribution of some tropical and subtropical marine algae' appeared. Svedelius distinguished between two groups, species common to the Indian-Pacific and the Atlantic oceans, separated by the Isthmus of Panama, and species common to the Indian ocean and the Mediterranean, separated from each other by the Isthmus of Suez, where open connexion had existed formerly. 'The great changes in the relation between land and sea can', he concluded, 'to this very day be traced in the present geographical distribution of the marine algae.' In a paper on the distribution of *Asparagopsis* other cases of disjunction were described (1934).

Contribution to the biology and ecology of vascular plants

Svedelius's interest in botany was by no means limited to marine algae. The plan he submitted to the authorities when he applied for the travelling scholarship that carried him to Ceylon included research in the biology of higher plants, and the tropics offered no end of interesting problems. His algological field work acquainted him with *Enalus acoroides*, a dioecious member of the Hydrocharitaceae which formed submarine meadows on soft bottom. The morphology was of course well known, but as no observations had been made on the living plant the mode of pollination remained to be discovered. It was supposed to be the same as in *Vallisneria*, but Svedelius was able to show that pollination in *Enalus* is a unique and quite fascinating procedure, due to ingenious devices in the flower structure and a close adjustment to the tidal changes of water level. The paper 'On the life history of *Enalus acoroides*' appeared in 1904. The second of the biological papers describes two cases of postfloral growth of the calyx in Convolvulaceae. Other types had been described before, and to these he added a new one, in which the fruit ripens in a water-bath excreted by the firmly closed sepals (1906). The third of his contributions to floral biology depicts the morphology of the inflorescence in *Lagenandra*, a peculiar type of Araceae, in which the ingenious adaptions to entomogamy has become meaningless and self-pollination the rule (1910). The fourth paper of the set, published in 1912, deals with the seed structure in *Dillenia* and *Wormia*. The latter had been merged into the former, but Svedelius showed that the seed structure in *Wormia* had been misunderstood and that the genus must stand.

By this time Svedelius had become absorbed in algological work; his first paper on *Delesseria* appeared in 1911. It happened, however, that twenty years later he cleared up the differences in the mode of pollination, due to the difference in habitat, between *Vallisneria spiralis* and *V. americana*. That his interest in many branches of botany was kept awake all his life is evident from his reviews in the Swedish botanical journal. We may of course argue that his contributions to biology carry little weight as compared with his pioneer work as an algologist, but they add to the picture of the keen and careful observer.

International contacts

A Swedish university man is fortunate in having long summer vacations which give him a welcome opportunity to make personal contact with his colleagues abroad. Svedelius visited practically all the important botanical institutions on the European continent and in Great Britain, including a number of the marine biological stations. In the spring and summer of 1912 he studied in the physiological laboratory at Leipzig under the famous Wilhelm Pfeffer, from March to July 1913 at the marine stations of Rovigno, Plymouth and Heligoland. He took an active part in the international congresses of plant science from 1905 to 1950 and represented the Swedish government at Cambridge and Amsterdam. The Ithaca congress of 1926 gave him an opportunity to visit American marine stations at Bar Harbor and Woods Hole. From 1935 to 1950 he was president of the botanical section of the International Biological Union. At the centenary of the University of London in 1930 and of the Botanical Society of Edinburgh the same year he was a delegate of the University of Uppsala. He represented the Swedish Academy of Science at the tercentenary of Isaac Newton's birth. His outstanding phycological work earned him international reputation and many distinctions. He was elected to the Royal Swedish Academy of Science and to the Royal Science Society at Uppsala in 1921—serving as its treasurer 1932-1952 and, on leaving this post, being made an honorary member of this body—and in 1931 he was made a member of the Physiographical Society of Lund. Among foreign learned societies which honoured him it will be sufficient to mention the Linnean Society of London (1937) and the Royal Society (1944). At the meeting of the British Association at Leeds in 1927, he was one of the foreign speakers. In 1930 he accepted an invitation from the University of London to deliver a series of advanced lectures.

Personality

In September 1898 this writer joined the botany class at Uppsala and met Svedelius for the first time. We had attended the same Gymnasium in Stockholm, but when I matriculated he had already left. Among the botany students he was one of the outstanding figures. With his splendid bearing and regular features he looked like an aristocrat; his social background was of the best; he had enjoyed a refined education, and his manners were perfect, but at the same time he impressed us as an intelligent and earnest worker who did not waste his time on idle pleasures. It never would have occured to him to kick over the traces, something which, indeed, could not be said of all of us. He was friendly and obliging, but a bit reserved; not that he showed any intention at all to isolate himself, but we felt the distance. I don't remember that, with the exception of the illustrious cytologist Otto Rosenberg (1872-1948), Professor of Botany at the University of Stockholm, he ever made any close personal friends among botanists. Two advanced students of jurisprudence, both later to become professors at the university, one of them in the end a member of parliament and a cabinet minister, the other Rector Magnificus of the university and finally Chancellor of the Universities, were his constant companions; the three had their meals and took their walks together, discussing social and political problems. For Svedelius, however, this was a side issue and did not in any way encroach on his scientific work. All his life, he took an active part in the Botany section of the Student's Natural History Society, rarely missed a meeting and never failed to give us an insight into the progress of his research, but he also had critical comments in store for other speakers. Loose talk and wild speculations made him frown. The session closed, we generally went to a restaurant for supper; as a rule Professor Svedelius came along, gladly joining in the discussions which ran high while the Swedish *punsch* sank in the bottles. In those days the restaurants closed at 11 p.m. sharp. It frequently happened that some of us resorted to some student's den to go on talking into the small hours, but Svedelius would bid us good-night and go home—and give us a chance to discuss university professors in general and ours in particular. How close our modest society was to his heart became manifest when, on the occasion of its 75th anniversary, he undertook to write its history (1946), a piece of both interesting and amusing reading. He was one of the founders of the Swedish Botanical Society in 1907 and an assiduous contributor to its journal.

I have the impression that Svedelius did not really love teaching and that the elementary courses bored him. But with his strong sense of duty he was a most conscientious instructor. His reserved disposition did not favour personal contacts with the younger set. One would have thought that this ought not to have prevented him from attracting the more advanced pupils and building up a school of phycology, as his colleague at Lund, Harald Kylin, did with remarkable success. But this did not happen. One reason may have been that so many students were attracted by two other Uppsala schools, one led by Rutger Sernander (1866-1944), professor of plant biology, with phytosociology, where he was one of the pioneers, as the favourite subject, the other under Oscar Juel. Administrative tasks and the financial affairs of the university took much of Svedelius' time, but he liked them. An interest in such matters was a heritage from his ancestors, and we used to say that if he had not been a prominent scientist he would probably have occupied some high civil service position. He was an influential member of the faculty of philosophy, its treasurer 1918-1938, attentively listened to in the senate and a member of its finance committee. To his official duties came his engagement in the Swedish Linnean Society, founded in 1917 to further the study of Linnaeus and his time. From 1924 to 1948 Svedelius was its prudent and foresighted treasurer. One of the principal objectives of the Society was to restore the old Linnean garden, situated in the centre of Uppsala and lying waste for the last hundred years. As its director, Svedelius was instrumental in its resurrection and left it a great attraction to visiting scientists from far and near. He also contributed frequently to the Society's yearbook.

At the statutory age of 65 Svedelius retired from his chair. He looked forward to this day with great expectations, for his health was excellent and his mental powers as keen as ever; he remained at the peak of his scientific activity. A burden had been lifted from his shoulders, he felt free and happy, and the sense of freedom worked a notable change in his disposition. He became much more sociable and accessible than we had been used to; he did by no means lack a keen sense of humour. Not that he was spared from sorrows. In 1914 he had married Miss Lisa Thegerström, daughter of the prominent painter Robert Th. and Elin Lamm. The marriage was a happy one, she took a lively interest in his work and helped him in many ways. Her death in 1955 at the age of 66 was a hard blow to him.

After his retirement in 1938 almost twenty years of active life were left him. He had always had the intention to return to a taxonomic revision of his Ceylon collection, the greater part of which was still unnamed. In the spring of 1956 the professor of Systematic Botany arranged a working room for him in the department of algology. Svedelius went there a couple of times during the summer, but he did not start work and his collection rested unpacked. Strength had begun to fail him, his legs refused him their service and the short walk from his home to the garden became too much for him. During his last years he was confined to his rooms. About a year and a half before his death I saw him for the last time. He looked remarkably well, his mind was clear, but he was unable to move and his speech was difficult to follow. He died on 2 August 1960, three days before his 87th birthday. One son and three daughters survive him.

C. SKOTTSBERG

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Abbreviations. *BDBG. Berichte der deutschen botanischen Gesellschaft.* Berlin.—*Bihang till KVAH* (Appendix to *KVAH*).—*BN, Botaniska Notiser. Lund.*—*KVAH, Kungl. Vetenskapsakademiens Handlingar* (Proceedings of R. Swedish Academy of Science). Stockholm.—*KVS, Acta Regiae Societatis Scientiarum.* Uppsala.—*SBT, Svensk botanisk tidskrift* (Journal of the Swedish botanical society). Stockholm.—*SBU, Symbolae Botanicae Upsalienses.* Uppsala.—*SLÅ, Svenska Linné-Sällskapets Årsbok* (Yearbook of the Swedish Linnean Society). Uppsala.

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H. T. Tizard

HENRY THOMAS TIZARD

1885-1959

Henry Thomas Tizard was born at Gillingham, Kent, on 23 August 1885. His father, Capt. Thomas Henry Tizard, R.N., C.B., F.R.S., was in the hydrographic service of the Navy, of which, in 1891, he was appointed Assistant Hydrographer. His mother was Mary Elizabeth Churchward. H.T.T. was the only son. He had two older and two younger sisters. His father lived until 1924 and his mother until 1931.

On his father's side, his grandfather was a ship owner and coal merchant at Weymouth. H.T.T. believed that the Tizards were originally Huguenots, who settled in Weymouth early in the 17th century. He had no record of their activities. On his mother's side, there is evidence of engineering among his ancestors. His grandfather was a civil engineer, first at Malta Dockyard, and later at Pembroke Dockyard. A relatively remote ancestor (1628-1700) on this side was Sir Paul Rycaut, F.R.S., (elected 1666). Little is known about him. He was an author and traveller.

H.T.T. had a good memory, particularly for the events of his early childhood—memories of the dockyard at Chatham, and the building of ships, and of sailors encouraging him to climb a rope ladder up a mast.

When his father moved to the Admiralty in 1891, the family lived at Surbiton, and here he went to his first school, 'Enfield House', kept by three maiden ladies. The mathematics master, Verey, he described as a magnificent teacher—to whom he owed more than he could tell. Indeed, he attributed much of such success as he had in later life to his grounding in mathematics at this school, where he remained for eight years.

Following the tradition of his father's family, he expected to enter the Navy, but in 1899, when due for the Entrance Examination, he discovered a blind patch in his right eye. A specialist said that it would probably clear, which it eventually did. But the Navy being now out of the question it was decided that he should sit for a Scholarship at Westminster. His good mathematical training at his Preparatory School earned him an Exhibition. He was soon awarded a Scholarship, without which it was apparently unlikely that the family finances would have enabled him to remain at Westminster.

He was fortunate in finding a good teacher of chemistry, though physics was sadly neglected. He paid a tribute to the teaching of mathematics, particularly to H. E. Piggott straight from Cambridge. He believed later that he should have been advised to try for Cambridge, where, in the excitement of the times following the discovery of the electron, he would have found the incentive which his temperament needed. He was apparently dissuaded by his housemaster from trying for a 'close' Scholarship at Trinity College, Cambridge, on the grounds that they were not given to scientists!

In 1903 he was elected to a demyship at Magdalen College, Oxford, and went up for the Michaelmas Term, 1904, when for the third time he found himself with a first-class teacher of mathematics, A. L. Pedder. He was placed in the First Class in Maths Mods. in 1905.

At this point the influence of his science teacher at Westminster, E. C. Sherwood, whom he regarded as a good teacher of chemistry though he knew very little physics, asserted itself, and he forsook mathematics for chemistry. Here his tutor was N. V. Sidgwick (later F.R.S.), of Lincoln, one of the strongest and most lasting influences of his life. Tizard's obituary of Sidgwick (*Obit. Not. Roy. Soc.* **9**, 237,1954) is an outstanding example of his command of language—a tribute to a great man rather than an obituary.

He sat for the Final Examination in Natural Sciences (Chemistry) in 1908. He remarked that chemistry at Oxford owed little to the then aged Professor. The University laboratories were in a poor state, and most of his practical work was done in the better equipped laboratories of some of the colleges (Balliol, Magdalen and Trinity). The practical teaching was good, but lectures were uninspiring. He learnt practically no physics. There were keen younger men on the staff, but there was little stimulus from the seniors. But with typical fairness, he recognized that his unfavourable judgement of the teaching of science at Oxford may have been due partly to shortcomings on his own part, bearing in mind the names and careers of his contemporaries.

It was during his Finals that, for the first time, he was sorely tried by ill-health. He had been satisfied with his performance in the theoretical papers, but the practical examination, taking the whole of the next week, found him with a high fever, and a temperature of 104 °F. He was removed to a nursing home, where the crude methods of those times succeeded in reducing his temperature but left him unable to tackle the paper on the second day, although he had the presence of a nurse to support him. He was grateful to the External Examiner for suggesting that he should go back to bed. On the third day he managed to put in three hours and on the fourth day the full six. In the list, he was given a First Class.

Sidgwick advised him to go to Germany, where Nernst had promised him a place in his laboratory. He managed to raise £130, from the College and from his father, which had to suffice for a year in Berlin. He spent the semester 1908-1909 there, Nernst having put him on to the condensation of acetylene to benzene. In the light of his results Tizard described it as a hopeless task, and he had much the same to say about another project on which Nernst put him early in 1909. It came to nothing. He had nothing publishable to show for his work, and decided not to risk the waste of another year. Nevertheless he recognized that he learnt a great deal of value in Nernst's laboratory. It was here also that he first met F. A. Lindemann.

He returned to Oxford and N. V. Sidgwick in the summer of 1909 and later spent some time in the Davy Faraday Laboratory at the Royal Institution, where he started work on the sensitiveness of indicators. Four of his papers appeared in the *Transactions of the Chemical Society* and his lecture to the British Association at Portsmouth on the subject was published in the *Report of the British Association* in 1911. In this year, he was elected to a Fellowship at Oriel, where for the first time he had found himself relatively well off. His total income amounted to something over £500 a year, which with free rooms and free dinner was affluence.

At Magdalen, during the summer of 1910, he met Kathleen Eleanor Wilson, daughter of Arthur Prangley Wilson, a mining engineer, and of Edith Alexandrine (Hill). Although it was not until 24 April 1915 that they were married, they had waited only for a suitable opportunity. Having no private resources, he was concerned about finance.

He suffered an attack of fever in 1912, similar to the one which had nearly robbed him of a First in his Chemistry Finals in 1908. But he nevertheless regarded life at Oxford as too comfortable and by 1913 had vowed that he would not stay longer than five years, and would then go 'into the wide world and earn an honest living'. He often wondered if he would have kept his vow if the war had not intervened. It was at this time that he first met what eventually became the greatest interest in his life, the part of science in national affairs, and particularly in defence. Hitherto he had taken no interest in the matter, although he shared the general excitement about the development of aviation.

There was first a short interlude. In the summer of 1914 he went to Australia as a member of the British Association. The Australian Government had provided free passages for about 100 well-known British scientists and, at the last moment, when one of them was suddenly unable to go, a friend suggested Tizard as a substitute. On board the *Euripides*, in addition to N. V. Sidgwick, were a group of great men of science—all Fellows of the Royal Society—Professor H. B. Dickson, Sir Thomas Holland, J. E. Petavel, Professor Seward and, above all, Sir Ernest Rutherford. Thirty years later, and eight years after Rutherford's death, Tizard broadcast one of the best of his Addresses, 'I knew a man—Lord Rutherford'. There were 13 years difference between them in age, but they formed a friendship on board the *Euripides* which ended only with Rutherford's death. Owing to the outbreak of war, Tizard had little time in Australia, but returned more than once in later years. The Commonwealth fascinated him, and he was the kind of man the Dominions understood and welcomed.

He returned to England early in October, and joined the Royal Garrison Artillery at Portsmouth in November 1914, but soon came to the conclusion that he could make a better contribution than by training territorials on anti-aircraft guns in Hilsea Lines. With the help of a friend, he managed to arrange a transfer to the Royal Flying Corps, as an Assistant Equipment Officer, 2nd Lieut., to undertake experimental work at the Central Flying School at Upavon, Wiltshire.

It was here that I first met Tizard. I was engaged in experimental work on aircraft in flight at the Royal Aircraft Factory, Farnborough (now Royal Aircraft Establishment), and the 'testing of the performance of service aircraft', which was a very broad description of the work of the Experimental Flight at the Central Flying School, had much in common with the Factory's work related to the design of aircraft. In the Commandant of the C.F.S., Commodore Godfrey Paine, R.N., Tizard recognized 'a very good commandant—he knew no science, but had a scientific spirit and encouraged experiment'. Among those whom Tizard had in his team were: G. M. B. Dobson (later F.R.S.), R. B. Bourdillon (whom Tizard had met in Oxford in 1911), and I. O. Griffith. Work in flight on the problem of the errors in sighting caused by the lag of the bomb behind a parabolic path, due to the resistance of the air to its flight, convinced Tizard that the error due to this cause was much less than the errors in flying the aircraft. He persuaded Paine that he could be of much more use if he learnt to fly himself. Permission was given 'so long as you go up only in weather which is too rough for cadets'. He qualified after 3½ hours dual instruction, and 2 hours solo.

Tizard loved flying as a pilot, and was generally in the highest spirits when in the air. Later he felt less confident and came to the conclusion that his uneasiness was due to too much flying over 15000 feet with no oxygen.

In January 1916 he became a fully qualified pilot, and flew to France where he met General Trenchard. The main purpose of the visit was to investigate the results of bombing. But by this time his interest was moving towards the wider problems of determining the performance of military aircraft, particularly in view of the improvements which were at last being made, and of the increasing importance of the aircraft for fighting and reconnaissance. Aerial photography and radio were developing. Cloud flying was becoming of great importance, and revealing new problems. He was much impressed by the first modern aircraft he flew, the Royal Aircraft Factory's BE2C, 'a wonderful machine to fly after experience with Maurice Farmans' (on which we both had learnt). 'It was completely stable, easy to fly in the clouds and easy to land.' He became convinced that these qualities were of outstanding value in military aircraft.

But it was also necessary to be sure that improvements in performance (speed, rate of climb, and ceiling) were real and not mythical. He was not content to rely on personal estimates about things which could be measured. He was not satisfied with the instruments available, and proceeded to stimulate the development of better equipment. G. M. B. Dobson devised instruments which measured directly rate of climb and the rate of consumption of fuel. Tizard also appreciated the great advantages of the new aircraft compass devised by Keith Lucas, F.R.S., at Farnborough. But he felt frustrated by what he regarded as the failure of the War Office authorities in London to appreciate the need for more experimental work, with the object of producing not merely better aircraft, but *better weapons*. They seemed to him to have little understanding of engineering, and even less of science.

Once again, towards the end of 1915, he met a man who permanently influenced his outlook on life. Bertram Hopkinson, F.R.S., had temporarily left his Professorship of Mechanical Sciences at Cambridge and joined the Department of Military Aeronautics at the War Office, with responsibility for the design and supply of bombs, guns and ammunition. Hopkinson made a profound impression on Tizard, an impression which deepened as time passed, and he came to know him intimately. Hopkinson's judgement was mature, he knew how to get the best out of people working under him, advised rather than ordered, and treated his staff as pupils rather than servants.

The two pages which Tizard contributed to Hopkinson's Obituary (*Proc. Roy. Soc.* A, **95,** XXVI (1919)) and his broadcast (5 March 1937) in 'Talks for Sixth forms', in a series entitled 'Someone who has influenced me in my life', are eloquent tributes to a man whose early death was a grievous loss to science, to engineering, and to aeronautics. For the first time, aeronautics was recognized by those in authority in the fighting Services as an essential element in defence. Its future, and possibly even the safety of the country, would depend upon finding the right man to head its research and experiment, a man in whom all would have confidence, on whose judgement they could rely in making the great decision—in Tizard's words—'What is best to do, among the so many things that might be done.'

Hopkinson had already decided on some things that *must* be done. The first was to move the whole of the experimental element at Upavon to more suitable surroundings away from the embarrassments inseparable from 'School' flying, and to provide better resources. In May 1916, he decided to move the Armament Experimental Flight to Orfordness in Suffolk, and in July 1916 to move the Aircraft Testing Flight to the adjoining airfield at Martlesham Heath. Tizard moved with them, was promoted to Captain, and made responsible for directing the scientific and experimental work of the whole station.

He moved his family to Woodbridge (his eldest son John Peter Mills had been born on 1 April 1916 and his second son Richard Henry was born on 25 June 1917 shortly after the move to Woodbridge). Although he was at Martlesham for only a year, Tizard looked back on his time there as one of the most enjoyable of his life. His brother officers and the staff at Martlesham were 'a happy friendly crew'. He was sufficiently experienced in flying as a pilot to get more than mere pleasure out of it and, with Hopkinson's enthusiasm and strength to stimulate him, he knew that he could make a significant contribution to the needs of defence in the air.

He flew a new Sopwith 'Dolphin' to France to be compared with the S.E.5's in the famous 56 Squadron. He noted that such advances in the understanding of aerodynamics as were made during the war had little practical effect on the performance of aircraft, compared with the development of the internal-combustion engine. This early appreciation of the overwhelming need to improve the output of an aircraft's power unit, per pound of its weight, determined the pattern of much of his later work for aeronautics.

Aeronautics had to wait some ten years for the full impact of two major discoveries in aerodynamics, though both were lying dormant during this time. Bearing in mind that these years included the 'hungry 20's' and the influence of the aftermath of war, it is fortunate that the inspiration of a few great men came to our rescue. From 1930 on it can fairly be said that both aerodynamics and thermodynamics advanced in parallel, producing in combination improvements which were of a different order of magnitude from what either could have achieved alone.

At Martlesham the front line was not far away. A Zeppelin raided Harwich and Ipswich. On 1 July 1917 Tizard took a new Sopwith 'Camel', with untested guns and ammunition, against a formation of German bombers attacking London. He managed to climb to their level in time to attack one of the rear machines from the tail. He fired a short burst without any visible effect—after which both guns jammed!

During 1917 he drew from his work at Upavon, and its further development at Martlesham, enough material to give to the Royal Aeronautical Society on 7 March 1917 a paper on 'Methods of measuring aircraft performance', primarily in order to emphasize that seriously misleading conclusions could be deduced from measurements, owing to the difficulty of allowing for variations in atmospheric conditions, particularly up or down currents. There was at the time much collaboration between the Testing Squadron, both at Upavon and Martlesham, and the Royal Aircraft Factory, where the same type of problem was met in similar experiments associated with the process of design. The following extract from Tizard's lecture sums up the views of both teams:

> 'So far as England is concerned, I believe that the general principles of what may be called the scientific testing of aeroplanes were first laid down at the Royal Aircraft Factory. Our methods of reduction were based on theirs to a considerable extent, with modifications that were agreed between us; they have been still further modified since, and recently a joint discussion of the points at issue has led to the naval and military tests being co-ordinated, so that all official tests are now reduced to the same standard. It should be emphasized that once the methods are thought out scientific testing does not really demand any high degree of scientific knowledge; in the end the accuracy of the results really depends upon the flyer, who must be prepared to exercise care and patience unnecessary in ordinary flying. Get careful flyers whose judgement and reliability you can trust and your task is comparatively easy; get careless flyers and it is impossible.'

In the Autumn of 1917 Tizard became restive. He felt unwell, and attributed this to the cumulative effect of high flying, 17000 feet or even higher, without oxygen. He had a blackout in an S.E.5 at 17000 feet due, not to high accelerations imposed by manœuvres, but to sheer lack of oxygen. He had just time to throttle back before he passed out. He came-to with the aircraft out of control at 5000 feet, but recovered rapidly and had no difficulty in righting the machine, and landed safely.* Aircraft testing, now routine, no longer appealed to him, so he turned his attention to cloud-flying. There was a proposal afoot to form a special cloud-flying bombing squadron in France and that Tizard should join it, though in what capacity is uncertain. Both this, and his uncertainty about the future of the Experimental Station at Martlesham decided him, in October 1917, to move his family from Woodbridge and to settle them in Oxford.

In the event, he was invited by Hopkinson to go to London as his Deputy. He became Assistant Controller, Research and Experiment, was promoted to Major and, in 1918, to Lt. Colonel. This was Tizard's first experience of what has always seemed to me to be his natural place in the world—Headquarters. However, it was not until 1920 that he took the step which ended in his abandoning science as a personal activity, and turned his whole mind and energies to science as a national asset and to the problems of its application to the needs of industry and of defence.

In the time since Hopkinson had first appeared on the scene at Upavon (November 1915) the Headquarters organization had changed radically. The responsibility for aircraft had passed from the War Office to the Ministry of Munitions, whose Minister was Mr Winston Churchill (now Sir Winston Churchill, F.R.S.). On the aircraft side the head was William Weir (later Lord Weir), Director General of Aircraft Production. Under him, the Head of the Technical Department was his brother Brig. Gen. James G. Weir, to whom Hopkinson was Deputy.

Tizard spent much time visiting Experimental Stations, usually by air, and flying himself. He learnt that, for a pilot who does not keep himself in *daily* practice, this method of transport is not to be recommended. His only real crash in his flying career was a forced-landing in an S.E.5 in the dusk, when he hit a tree in a field, finding it a convenient shock absorber.

His attention was inevitably drawn to the serious situation which arose during 1917 owing to the attacks of German submarines on tankers bringing in petroleum fuels. The shortage of aircraft petrol became acute, particularly because it was derived entirely from Pennsylvania oil. For some reason, which apparently nobody understood, this was particularly suitable for aircraft engines. It consisted almost entirely of aliphatic hydrocarbons. Tizard decided that tests should be made at Martlesham with a mixture of standard aircraft petrol and benzol from gas-works. Everyone was surprised to find that this mixture suited aircraft engines even better than the standard aircraft petrol. Unfortunately the benzene in gas-works benzol tended to freeze out of the mixture at low temperatures, although other aromatic compounds did not. But there was a fair supply of toluene from Burma. Most of this was used for explosives, but enough of the residual aromatics could be spared for the fuel for aircraft engines. This almost accidental discovery was largely responsible for Tizard's continuing interest in fuels for aircraft engines, and so for one of his most important contributions to aeronautics, of which a fuller account is given below.

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* When I read this note, I thought of my own experience. I seldom reached 17000 feet in 1916-1919. But by 1941 oxygen, like the parachute, was available in all service aircraft. I was trained to breathe it from the ground upwards—just to be sure that the system was working, and that lack of oxygen did not make me forget to turn it on!

By the middle of 1918 doubts about the outcome of the war had fallen to a point where planning for life after the war had become active. The importance of aeronautics to defence was now recognized. We could no longer afford to ignore it in peace-time, since to do so would place us in a position of great danger in the face of a threat of another war. Moreover, its significance for the future prosperity of the country, as a means of transport in peacetime, was at last becoming a matter for action.

The outstanding success of Hopkinson, in stimulating and directing research and experiment, had led to the preparation of plans for the establishment of a National School of Aeronautical Engineering of which he was to have been the Head. But he had learnt to fly and, for the same reasons as persuaded Tizard, he used aircraft as a means of transport, piloting himself partly because he enjoyed it, and partly because it helped him to realize the things that matter in the air. On 26 August 1918 when returning from Martlesham in poor weather which deteriorated to complete cloud-cover over London, it seems that he tried to descend through the clouds, lost control and probably allowed the aircraft to spin from which he could not recover.

It fell to Tizard to take over Hopkinson's position in London. He was however attacked by the influenza plague, which was widespread in the Autumn, and did not recover until December. He was demobilized in the Spring of 1919, and returned to Oxford.

In later years we sometimes talked about the influence of war on our lives. We agreed that war simplified everything, because all personal plans, ambitions, likes and dislikes become of little significance, compared with the one overriding objective. But for both of us there was a difference between 1914/1918 and 1939/1945. In company with many others, I had found far greater stimulus in the second war than in the first, mainly because in the second aviation came into its own, in spite of the neglect from which it had suffered in this country until a date which was far too near to 1939 to be comfortable for anybody. For Tizard the second war led in some ways to personal disappointment, though I do not believe that he felt that he had been misused or ignored. The material from which much of this memoir has been composed was written by him in or shortly before 1959, the last year of his life. I can find in it nothing to justify a different conclusion. Of the period 1935 to 1952, however, with all its tortuous paths of assertion and contradiction, rumour and denial, I am less qualified to speak from personal knowledge than is Professor R. V. Jones to whom I am greatly indebted for his account of this part of Tizard's life.

But for the first war let Tizard speak for himself: 'The war did me a great deal of good. It pulled me out of the ruck at Oxford. It forced me to do things which I should never have done of my own accord in peace-time, but which I found in practice much easier than I imagined. It brought me into close and friendly contact with all sorts and conditions of men and it made me realize that a purely scientific education *was* of value for men who had to deal with the practical affairs of life. Of course I was one of the lucky ones.'

There was, however, one difference between the two wars which affected all who took part in both, and that was in our relative ages. At the end of 1918 Tizard was 33. Twenty-seven years on, at the end of World War II, the prospect which faced us seemed very different—obscure, with a darkening horizon. We were in much the same position as everyone else, but we noticed that the younger men worried less than we did.

Be that as it may, after the first war, Tizard returned to Oxford, to his Fellowship at Oriel and to chemistry. He committed himself, with some doubts, to give a full course of lectures in physical chemistry which, judged by remarks made to him in after years, were successful.

He resumed his research on chemical indicators, but his most successful and most valuable research was on the influence of the chemical composition of the liquid fuel on the thermodynamics of the spark-ignition engine. Tizard's interest in this problem was first aroused by his experience at Martlesham in 1917, mentioned above. Owing to the great importance of this question to the fuel-oil industry, the interest of Sir Robert Waley-Cohen of the Asiatic Petroleum Company (Shell) had been attracted by the parallel work of H. R. Ricardo (later Sir Harry Ricardo, F.R.S.). The outcome of this was very substantial financial support from Shell for a programme of research, at a laboratory set up by Ricardo at Shoreham. In this Tizard participated, bringing with him D. R. Pye (later Sir David Pye, F.R.S.), who had been a member of the team at Martlesham.

The relationship between the three leading spirits, and in particular the parts played by Tizard and by Pye, are admirably described by Sir Harry Ricardo in a letter to Sir Harold Hartley, F.R.S., from which I have their permission to quote below:

'Our terms of reference from the Shell Co. were to investigate all the properties of a fuel which could influence the performance of a spark ignition engine and to evaluate their relative importance, that of detonation being only one. I told Sir Robert that to do this we should need the help of one or more really first class physicists and he readily approved my suggestion that we should retain Tizard and Pye as consultants. Tizard stipulated that the results of any such investigation should be published; to this Sir Robert generously agreed provided that no publication was made until 18 months after the Shell Co. had received our report.

'It would have been impossible I think to have found a more ideal collaborator than Tizard. He entered into the project with zest and was always full of valuable and ingenious suggestions.

'During the last year of the war Tizard and I were in constant touch discussing how we would set about the research and what equipment we would need. We found ourselves in complete agreement in mapping out a plan of campaign to be started as soon as the war was over and our laboratory was ready. It had long been my ambition to have a really versatile research engine in which, among other things, the ratio of compression could be varied while running and therefore without disturbing any temperature or other physical conditions, and I had already prepared scheme designs for such an engine. Tizard agreed with this but advocated that we should design and build another unit using the same variable-compression cyclinder and mechanism but in which the piston would make only one rapid stroke and then be locked at top dead-centre. With this machine, with the help of Hopkinson's optical indicator and by finding the critical ratio of compression for each fuel, we could study the unstable conditions when on the verge of spontaneous ignition. This machine, always known as the "Sphinx" because of its peculiar shape, proved very useful as an adjunct to the variable-compression engine.

'All this was in 1917-1918. The war was still on and all of us were engaged on various war services. We had however many opportunities to meet and discuss matters.

'It was agreed that as soon as the war ended and we were released from our various duties, Tizard and Pye would return to Oxford and jointly prepare a very full thesis setting out all the physical characteristics of all the light hydrocarbon fuels and also other possible volatile liquid fuels such as Alcohol, Acetone, Ether, Carbon Disulphide, etc., while I and my colleagues would supervise the building of our new laboratory at Shoreham and the construction by Messrs. Peter Brotherhood of both the variable-compression engine and the "Sphinx" together with the collection of such other test equipment as we should need. By the summer of 1919 all was ready. Tizard and Pye had prepared a really monumental analysis of all the physical properties of the fuels such as specific heats and dissociation of the gases before and after combustion at all temperatures up to 2500 °C. For this they drew on all the known data supplemented by their own researches at Oxford. In the course of it they investigated all such factors as the total energy per cubic inch of combustible mixture, the latent heat of evaporation, the change in specific volume before and after combustion, the temperature coefficient, etc., and pointed out how each and all of these factors might be expected to influence the performance of an engine, some of which, notably the very important influence of the latent heat, had not at that date been appreciated.

'By the summer of 1919 all was ready to start work. The variable compression engine was installed together with the necessary test gear. The Shell Co. had provided us with all the samples of both commercial fuels and pure substances we needed, together with a bulk supply of a specially prepared fuel from which all the aromatics had been removed to serve as a basis of comparison with all the others.

'Tizard came to Shoreham and we all set to work in real earnest. Brotherhood's had made a superb job of the variable compression engine and it proved a really precious instrument, not merely as a knock-rating unit but for evaluating the importance or otherwise of the other physical properties of the fuels, some of which had a more important influence on the performance of the engine than any of us had anticipated. For the next eighteen months Tizard spent most of his time with us at Shoreham while Pye remained for the most part at Oxford, but none the less spent several months with us at Shoreham during which he concentrated on the "Sphinx" to which he had become very much attached, and which, some years later, he took with him to Cambridge.

'It is difficult to point to any one achievement of his (Pye's) except of course his analysis of the properties of fuels, but Tizard's help in devising ingenious tests and his astuteness in analyzing the results and drawing from them the right conclusions were invaluable. For the most Thornycroft and I carried out the actual engine tests and Tizard and Pye the analytical work. As had been expected we confirmed that the incidence of detonation was the most important single factor limiting the performance of the petrol engine, and, at Tizard's suggestion, we expressed this in terms of Toluene Number, that is to say, the proportion of toluene (the least prone to detonate of any hydrocarbon fuel we had tested) which had to be added to η heptane (the worst) to match the fuel under test, for we had established a straight-line relationship in the blending of these two. Several years later the Americans substituted iso-octane for toluene and today the expression "octane number" has become universal.

'During the course of this research we all learned a great deal about the behaviour and appetite of the petrol engine and were able to advise the Shell Company as to its diet.

'By the end of 1920 our programme of research was almost completed and from Tizard's point of view was losing its scientific interest, for it was degenerating into the routine testing of shipments of fuel from various parts of the world. The upshot of all this research was then published very fully in two series of articles "The character of various fuels" by Tizard and Pye (1924) and "The influence of various fuels on the performance of the petrol engine" by Thornycroft and myself. Shortly after this Tizard suffered a very severe attack of influenza, which put him out of action for several months, and that was the end of our active co-operation though we kept in close touch as friends for the rest of his life.

'As a collaborator on research of this kind Tizard was quite ideal—he had a vast fund of knowledge, was full of ingenious ideas backed by a thoroughly practical outlook. As a friend his personal charm and sense of humour was always a delight and he was never dull.'

Tizard himself summed up the results of the work as having established the following three main conclusions:

1. At a given compression ratio, the nature of the liquid fuel did not materially influence the maximum power developed. In practice, the best fuel was that which showed the least tendency to knock.

2. The efficiency of the engine, measured by the specific fuel consumption, was highest with a weak mixture.

3. The maximum power developed, at a fixed compression ratio, was constant over a wide range of rich mixtures. This was explained by the dissociation of carbon dioxide at high temperatures.

Tizard noted that it was interesting to reflect that 'but for the dissociation of carbon dioxide, it would have been very difficult, if not impossible, to design a practical multi-cylinder petrol engine'.

During 1919 and until September 1920 Tizard spent a good part of his leisure time on this work, which resulted in six published papers, mostly in collaboration with others. The most striking products of the later phase of this research were published in papers whose authorship is shared with Pye. Indeed, in this work, probably the greatest contribution of either of the authors to an advance in understanding of a problem of high significance, both theoretical and practical, the names of 'Tizard and Pye' are regarded as inseparable.

In February 1920 he was appointed University Reader in Thermodynamics (Chemistry) and in April was re-elected for seven years a Fellow of Oriel. It seemed that he was settled in Oxford after all. But once more he was forced to consider whether his feet were set on the right path. Within a few months he was invited by Sir Frank Heath, the Secretary of the Department of Scientific and Industrial Research, still a relatively new Department, to join him as an Assistant Secretary. He had doubts about the suitability of the Oxford climate for his tiresome and uncertain health, which made him think hard. But what really decided him to accept was that he had come to the conclusion that he 'would never be outstanding as a pure scientist—younger men were coming on—all with greater ability in that respect'. His war experience had taught him the immense importance of the application of science to national affairs. The financial problem of educating his children weighed on his mind, and he decided it was better to have a relatively stable position, a good salary with prospects, than a Readership, whose emoluments were only about half as much, though he could (and at that time in fact did) augment them by an equally remunerative consultancy.

He stayed long enough in Oxford to examine in Final Schools in 1920, 'when I gave First Classes, *inter alia*, to C. N. Hinshelwood, E. J. Bowen and Sydney Barratt (not a bad lot!).'

Tizard resigned his Fellowship on 30 June 1920, leaving with a 'lively feeling of gratitude to Oriel', and moved his home to Wimbledon. He suffered another sharp attack of influenza, seemed to recover fairly quickly, and then went slowly downhill for months. He was well enough to join the D.S.I.R. in September 1920.

His summary of the origin and background of the D.S.I.R., and its history up to the time when he joined, is interesting not only for what he says of the Department but for the light it throws on the way in which his mind grasped a problem of this kind. He regarded Germany, at the beginning of the century, as the most highly educated country in the world, supreme in science, and with a lead in chemistry so long that it seemed impossible ever to catch up. In the crucial twenty years before 1914 there had been few signs of any appreciable influence of science on the mind of Government in England. Within three weeks of the outbreak of War the Board of Trade had appointed a Committee, under Lord Haldane, 'to consider and advise as to the best means of obtaining, for the use of British industries, sufficient supplies of commercial products, colours and dye stuffs of the kind hitherto imported from countries with which we are at present at war'. But little scientific work of importance to industry could be started in the years from 1916 to 1918.

Early in 1920 it was decided that steps should be taken to co-ordinate the scientific work of the Defence and Civil Departments, by means of 'Co-ordinating Research Boards'. Four Boards had been established, for Chemistry, Physics, Engineering, and Radio Research. Their Chairmen were: Sir Richard Threlfall, F.R.S., Sir Joseph J. Thomson, F.R.S., Sir Charles Parsons, F.R.S., and Admiral Sir Henry Jackson, respectively. Tizard's task in 1920 was to take charge of these Boards. Their terms of reference were 'to organize the scientific work that was devoted to the fighting services so as to avoid unnecessary over-lapping, to secure the utmost economy of personnel and equipment, to facilitate the interchanging of scientific knowledge and experience between all the Departments concerned, and to provide single direction of financial control for all work of a fundamental nature of civilian as well as military interest'. It is not surprising that Tizard regarded these terms of reference as a mistake. How could committees in civilian departments organize work for the fighting services? So far only one service department—the Admiralty—had appointed a Director of Scientific Research and that not until 1920. This was F. E. Smith (later Sir Frank Smith, F.R.S.). Tizard's personal experience of research in war time, and of its relation to a fighting service, had given him clear ideas of how such work should be organized. There should be a Director of Scientific Research in *each* service, who would exert his influence over the whole service, who would ensure that the Research and Experimental Establishments studied the true needs of the service, and that their effort was centred on things that mattered. He was almost quoting Hopkinson.

In due time, the 'co-ordinating' functions of these Boards were abandoned, and simpler, less formal, methods adopted.

Tizard took a keen interest in the Radio Research Station at Slough whose Director was R. A. Watson Watt (later Sir Robert Watson-Watt, F.R.S.). It was from the work done there that radar emerged in 1935.

He was instrumental in obtaining from the Physics Board a grant of £500 a year for the work being done at the Cavendish Laboratory by Dr Kapitza (later F.R.S.), under Lord Rutherford, on the production of magnetic fields of very high intensity.

On the service side, he continued to press the Departments to take a more enlightened view of the functions of a Director of Scientific Research. He argued the point with Sir Geoffrey Salmond, the Member of the Air Council for Research and Development, who offered him the post of Director of Scientific Research at the Air Ministry, which he declined because he considered the salary was too low. Moreover, the terms of reference excluded armament and bombing, an omission to which he attributed the inadequacy of our bombs in 1939. He was convinced that the Directors of Scientific Research of the services were never used in the right way in peace time. In this he may well have been right. But nearly all his experience of research had been in war, when the relationship between Headquarters in London and the Services' Research Establishments was more flexible and less irksome than it became in peace. Perhaps he did not regret that the problem which he now faced was apparently relatively straight-forward and simple—the peace-time direction of Government-maintained or Government-aided scientific research in aid of the country's industries. But it is doubtful whether he found it so.

In October 1922 he became Principal Assistant Secretary, and in June 1927 he succeeded Sir Frank Heath as Permanent Secretary. During these years he became aware of the causes of the difficulties which faced the Department. He came to value the Advisory Council and made friends of its members. He had a particular regard for the Chairman, Sir William McCormick, F.R.S., of whom, in collaboration with Sir Richard Threlfall, F.R.S., he wrote an outstandingly fine obituary notice, which contains probably the best history which exists of the State's growing interest in, and support of, scientific and industrial research. Tizard did not agree with Sir Joseph Thomson about the kind of research which Industrial Research Associations should undertake, but he regarded him as the outstanding Member of the Council, to whom everybody deferred. Sir Richard Threlfall became his closest friend on the Council—'took me to Vyrnwy and taught me to fish—far bigger and wiser than most public men I have met—all my impressions of him are happy'. To him Sir William B. Hardy, F.R.S., 'with his black beard and commanding air, looking rather like an Elizabethan pirate, was one of the great men of his time. I never met a man so exactly fitted by knowledge and intellect and force of character to be a leader in industrial research.' Sir Joseph E. Petavel, F.R.S. was Director of the National Physical Laboratory—'the best I think the N.P.L. has ever had. But then he was an engineer, the only engineer ever appointed Director N.P.L. The post is officially the gift of the Lord President, but in practice is left to the Royal Society to appoint, which prefers a physicist.'

He was beginning to feel sure of himself in his new environment. He had been elected a Fellow of the Royal Society in 1926 and in 1927 was awarded the C.B. His health still worried him, but after an attack in 1922 (not long after the birth on 15 May 1922 of his third son, David Andrew Thomas) he fortunately consulted a doctor who told him he could find nothing wrong with him except that he was worrying about his wife and family. From this time onwards until about 1950 it seems that his health did not trouble him so much.

Shortly before he left Oxford in 1920, Tizard had been invited by the Secretary of State for Air to join the Aeronautical Research Committee (since 1946 'Council') as an independent member. In 1921 he became the official representative of the D.S.I.R. on the Committee. His membership was almost continuous for the following twenty-two years. Indeed, though the field of his interests and influence inevitably widened, his early association with aeronautics, in years when flying itself was still in its infancy, provided an invaluable foundation of practical experience for many of his most significant contributions to the problems which faced the country.

All of these had one element in common. We were signally failing to make fruitful use of the knowledge which our own research was providing. We were falling behind other countries. We had men of science, with power of insight and judgement, but they lacked influence. Of Tizard's influence on aeronautics there was never any doubt. On 29 May 1925 he gave the Wilbur Wright Lecture before the Royal Aeronautical Society on 'Fuel economy in flight'. It showed a remarkable grasp of a complex problem, and a characteristic power to pick out the points in which substantial progress in the whole aircraft could be expected, in order to put the significance of possible improvements in fuel economy in proper perspective.

In the same year, on 2 October 1924, he became Chairman of the Society (a position which in 1927 was combined with that of President) and delivered an Inaugural Address with one of the shortest and most striking titles which the Society has ever known—'Commonsense and aeronautics'. He described the title as 'provocative' but explained that in this context 'commonsense' had its literal meaning 'the intelligent thoughts of the ordinary man'. He asked and answered:

> 'What does the ordinary intelligent taxpayer think about aeronautics? Perhaps his chief thoughts can be summarized as follows:
>
> (1) The Air Force is a necessity; but let it be as efficient and economical as possible.
> (2) Civil aviation is a luxury.
> (3) Flying is unsafe.'

The economics of air transport, and its relationship to defence, were summed up in words which might have been written of the present day:

> 'All activities that consume more than they produce are luxuries, unless they bear within them the promise of greater productivity in the future. We are passing through a period when every item of unproductive national expenditure has to be scrutinized with more than usual care. That expenditure on civil aviation has escaped a really critical scrutiny I attribute to two causes—one that many people have a vague idea that it is a necessary factor in the aerial defence of the country; the other that the public mind is preoccupied with more important matters. All the more reason why we who

honestly advocate expenditure on civil aviation should examine our motives and methods and ask ourselves whether they can be justified . . . The majority of you will probably agree that only on one condition will civil air transport become a real part of the economic life of the nation, and at the same time a real factor in the defence of the country. The one condition is that it pays its way. So long as it is carried on at a loss, so long will it be negligible for defence purposes. Machines developed for civil purposes will never be of more than trivial importance in aerial warfare; what will be of importance is the development of a large and healthy industry, and the adoption of aircraft by a considerable section of the community, either as a normal means of transport, or as an indispensable adjunct to other industries. Neither of these events will come about until civil aviation pays its way.'

He added that:

'If you accept these statements, there is only one logical conclusion, It is that air transport cannot yet be regarded as a business; it is simply a large scale experiment. It may be regarded as a natural and necessary consequence of research and experimental work in the laboratory and workshop . . . All industrial research that has a promising issue on the small scale must be tried out carefully on the large scale before its conclusions can be finally adopted. Often it is only on the large scale that those modifications and improvements are introduced which make the difference between success and failure. For air transport to have any prospect of success, it must be operated on the same sound principles that many years' experience have shown to be essential in the conduct of other industrial research.'

Finally:

'Flying is unsafe. Here the commonsense view is undoubtedly right. It *is* unsafe. Not dangerous but unsafe. About as unsafe, let us say, as mountaineering, but hardly as unsafe as motor-racing. It is no good trying to conceal from oneself, and from the taxpayer, that air transport is not a perfectly safe means of transport when we know that it must be made 100 times safer before it can be a reasonable commercial proposition. Considering the nature of the aircraft engine, it is astonishing how reliable it is; considering the newness of the science, it is astonishing how stable and strong an aeroplane can be made; but it is equally true to say that no effort must be spared in making the one more reliable, and the other more stable and foolproof.'

In 1933, Tizard was appointed Chairman of the Aeronautical Research Committee. He was unquestionably the best man for a task which rapidly became both difficult and important. Aeronautical activity in the country expanded rapidly in face of the threat of war. By 1936 we were trying desperately to make up for lost time, to improvise means of doing what we ought to have done five or ten years earlier. To some it might seem an odd time in which to undertake research, and to seek the guidance of a committee which was not apparently designed to stimulate action, or to make recommendations on urgent matters of policy when the evidence was inadequate and the future obscure. It is, however, precisely in such times that a man with Tizard's powers of judgement and leadership is invaluable. It was a time when it was overwhelmingly important that we should choose right among the many things we were invited or urged to do. He showed several times in his life that he could lead opinion in fields in which he could hardly claim to be a professional expert.

A similar problem had faced Bertram Hopkinson twenty years before, as Tizard showed in his broadcast on 5 March 1937 (see p. 317, line 11). Tizard was not, as Hopkinson had been, at the head of Research and Experiment. But he knew that as Chairman of the Aeronautical Research Committee he was possibly more strongly placed. His understanding of aeronautics was deeper than Hopkinson's had been, his experience was greater, and his power of judgement was outstanding. There were times when he seemed to regard the Committee as a forum in which to test his own ideas. But the recommendations were the Committee's and with a Chairman of Tizard's personality and reputation it was often possible to ensure action before it was too late.

I have vivid memories of these years with Tizard. I was a member of various Sub-Committees of the A.R.C. from 1920 onwards. and of the Engine Sub-Committee (of which he was almost perpetual Chairman) from 1928, so that I never lost touch with him. I was a member of the A.R.C. from 1930 for most of the years until 1946, and saw in him in action at this level.

Using the words of Dr R. S. Hutton:

> 'I was immensely struck with the way in which Tizard handled this Committee. It was at a time when rapid and great strides had to be made in the speed and size of aeroplanes. After briefly outlining some immediate problem as he saw it, he asked for proposals from the experts. When no one was prepared to start, Tizard himself gave an imaginative sketch of some ideas and then members of the Committee one after another responded. I remember this vividly and always have felt that it was this contribution of Tizard's which really led the way to some of the big advances on the material side of flying which led to our success in the Battle of Britain.'

In October 1929 Tizard resigned from the Department of Scientific and Industrial Research and became Rector of the Imperial College of Science and Technology, an appointment which he held until 1942. His period of 'war service' began in 1935, and his Chairmanship of the Aeronautical Research Committee lasted until 1943. He carried the heavy load of three distinct tasks without signs of distress, and did some of the best work of his life in all fields.

At the College he set an example to all. After more than a full day's war work, he would get back to the College at midnight, rouse the Secretary, Mr G. C. Lowry, from his bed, and work until 3 or 4 a.m. giving decisions which, to quote Mr Lowry, 'almost without exception proved absolutely right. He was probably the first Rector who combined with intellectual ability and great personal charm such a high degree of organizing and administrative capacity, with a knack for quick understanding and a willingness to enter into detail.'

On the question of Tizard's qualities as an administrator, about which the professional historian may well find it difficult to judge in the light of critical remarks which have appeared in the Press, it should perhaps be said here that most of these can be traced to a failure on the part of the critics to agree on a definition of 'administrator'. It is difficult to believe that Tizard regarded himself as a born 'superintendent' or a good 'manager'—a 'director' perhaps, a 'boss' never, a 'chairman' undoubtedly. He was a master of the searching question*. He had the patience to listen, the power to think, the ability to decide, the courage to change his mind. He had little interest and less belief in size for its own sake†. But he was convinced that the Imperial College was destined to be the first of the Technological Universities upon which the future prosperity of the country would largely depend, and he spent himself unsparingly in devising ways of securing a sound foundation for its future expansion. He was constantly looking ahead.

In Mr Lowry's eyes he was clearly a hero, and to their Secretaries few men are heroes. But it is probably in the words of Dr H. J. T. Ellingham (Fellow of the Imperial College, and now Secretary of the Royal Institute of Chemistry) that we find the best appreciation of his strength and weaknesses.

'Obituary notices and other communications that have so far appeared have not brought out fully all the qualities of Sir Henry Tizard, and some of the characteristics not mentioned, or referred to only obliquely, may be among those that made his tenure of office as Rector of Imperial College so successful and memorable. Indeed it was during his wise administration of its affairs that the College took an important leap forward, and its present status owes much to the seeds that he sowed.

'Tizard had the great gifts of being able to talk on seemingly equal terms to people of all kinds and ages, and to find out what they thought about things. The views of professors, of students—and of office messengers—on matters relating to their jobs (or on other topics) appeared to be of absorbing interest to him. He regarded each as an individual worthy of his attention, and he had a happy knack of throwing out the challenging remark that would stimulate response. It is not surprising therefore that he kept his finger so effectively on the pulse of the organization.

'Though a firm disciplinarian when occasion warranted, he found it difficult to keep the twinkle out of his eye when the case had been settled. This does not mean that he suffered fools gladly: far from it, but he was still less well disposed towards the pretentious and those who sought to claim unjustified privilege. For himself he claimed nothing more than the bare necessities for carrying out his duties, and at one time during the war was horrified to discover that an official car was being kept continuously at his disposal; thereafter, for ordinary journeys he used a 'bus, and hoped that the chauffeur would be given more useful employment.

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* I owe this to Sir Basil Schonland, F.R.S.

† I can hear him now, on one of his infrequent visits to Farnborough in 1941-1946, when I was the Director of the Royal Aircraft Establishment—'Places like this are far too big. Not more than 500, all told, including the cleaners—if you want to do any research.'

'Tizard had a flair for inspiring people with some of his own enthusiasm for getting things done. The encouragement that he gave to younger men in the Services, as well as in academic life, is perhaps his greatest contribution to progress. The fact that he did not always work through the "usual channels" sometimes caused heartburnings in the upper hierarchy, but even this was perhaps a useful stimulus to entrenched authority. In any event, he liked shaking up the complacent and he had no compunction in doing so during the war.

'Mention has already been made of Tizard's originality of mind. He was always throwing off ideas about all sorts of things, and producing in those who picked them up an urge to put them into effect. Even in the darkest days (and nights) of the war, when Tizard carried so many heavy responsibilities, his flow of ideas about the work of the College and its future was uninterrupted. Indeed, even when laid low with one of his occasional bouts of influenza a series of illuminating jottings would appear on a bedside pad. And Tizard's ideas were seldom commonplace. Many were highly unorthodox, or at least had an unusual twist to them; some that turned out to be impracticable he was ready to discard, though with some reluctance if he thought that they were of a kind that it would have been fun to try.

'Faced with alternative courses of action Tizard was usually prepared to make a quick and clear cut decision. It may be that his choice was sometimes influenced by a "hunch", but the workings of his mind were so well integrated that his hunches were often as well based as the carefully thought out conclusions of other men. At any rate most of them worked, and it certainly seemed that he had an innate capacity for seeing things in their true proportions—even through brick walls.'

I have been fortunate to enlist the help of Professor R. V. Jones, C.B., C.B.E., who has written the story of Tizard's work from 1935 to 1952 (which follows). Of this period Professor Jones had intimate personal knowledge owing to his position then in the Air Ministry, on the Air Staff, as Director of Intelligence, and as consultant to the Ministry of Defence.

TIZARD'S TASK IN THE 'WAR YEARS' 1935-1952

BY PROFESSOR R. V. JONES

After 1933 it became increasingly obvious that the policy of Nazi Germany was likely to lead to war, and that Great Britain would have to defend herself against the attack of the German Air Force. There had been informed pressure, led by Winston Churchill and F. A. Lindemann, against Baldwin's counsel of despair in 1933 that 'the bomber will always get through'. As a result, a renewed scientific appraisal of the problems of air defence would almost certainly have taken place in one form or another—the national need was far too strong and too widely felt to be ignored.

In the event, two distinct bodies were set up to undertake the appraisal. The first was the Committee for the Scientific Survey of Air Defence, conceived by the Director of Scientific Research of the Air Ministry, H. E. Wimperis, and his scientific Assistant, A. P. Rowe. They advised the Secretary of State for Air to invite Tizard, already well known for his qualities as a stimulating chairman with a long experience of aeronautical technology and test flying, to be chairman of this new committee, which met for the first time on 28 January 1935. The second body to be formed was the Air Defence Research Sub-Committee of the Committee of Imperial Defence. This first met on 10 April 1935; its formation, which was probably directly due to the pressure of Churchill and Lindemann, had taken longer than that of the C.S.S.A.D. Tizard was appointed a member of the C.I.D. Sub-Committee from the start; as it worked out, this body became more concerned with the political and military decisions required to implement the air defence programme of research and development, while the C.S.S.A.D. (the 'Tizard Committee') was concerned particularly with the research itself.

The original independent members of the committee under Tizard were Professor P. M. S. Blackett, F.R.S., and Professor A. V. Hill, F.R.S.; Wimperis was also a member. Lindemann was invited to join the committee, but he delayed accepting, on the advice of Austen Chamberlain; after his position had been clarified in a Parliamentary Debate on 7 June 1935, he accepted the invitation. It is probable that Lindemann could not overcome the suspicion (which appears incidentally to have been unfounded) that the Air Ministry and Tizard had conspired to frustrate his own proposal to place the scientific appraisal directly under the Committee of Imperial Defence. He argued that since the Air Ministry had given Baldwin the notorious advice only a year or so before that the bomber would always get through, it was too committed to take a fresh viewpoint. His membership of the Tizard Committee was therefore thoroughly uneasy, both for himself and for the other members of the Committee, and procedure became so difficult that in July 1936 Blackett and Hill resigned. The Air Ministry thereupon re-formed the Committee without Lindemann, whose behaviour had been unquestionably awkward, although it must be said that his contributions were much more positive than some accounts have made out.

The unhappy quarrel between Lindemann and Tizard cannot be ignored, but it should not be allowed to loom out of perspective. It is not clear whether the air defence controversy was the original cause of the trouble between the two men, or whether the difference had started earlier. Tizard himself recorded in his unpublished autobiography that he thought that Lindemann had been previously upset by not being appointed to the Aeronautical Research Committee. Here Tizard's usually good memory appears to have been at fault, since the records show that Lindemann was in fact a member of that Committee, and Tizard—in trying to explain to himself the cause of the quarrel—seems to have imagined an explanation contrary to the facts. This may well be evidence of the pain that the quarrel caused him.

Shortly before the Tizard Committee first met, Wimperis had asked R. A. Watson Watt, then Superintendent of the Radio Division of the National Physical Laboratory, to look into the possibility of destroying aircraft by projecting a beam of electromagnetic radiation. Watson Watt replied in a note which dismissed this possibility on energy considerations, but which went on to hint that there might be enough energy reflected from an aircraft, with radio field strengths that could readily be created, to give an echo that could be detected by a radio receiver on the ground. Wimperis told the gist of this note to the Tizard Committee at its first meeting, on 28 January 1935; Watson Watt produced a full memorandum on 12 February, and a practical trial was urgently and successfully made near Daventry on 26 February. This success transformed the prospects of defending Britain from air attack, since it promised to fulfil the first requirement of a defence system, that the course of the attacking aircraft should be known accurately and quickly enough for it to be intercepted.

Radar, or R.D.F. as it was then called, would have come into existence irrespective of the formation of the Tizard Committee: the state of radio technology was ripe, and the need was obvious. Within a short time radar was invented in several different countries. The fact that its operational employment proceeded at its greatest rate in Britain was due in part to the facts that our need and our opportunity for a successful defence system were greater than those of any other country. Having said this, we must also realize that despite the need and opportunity, the development and employment of radar could very easily have gone wrong: the early apparatus was far from reliable and it was to some extent an act of faith to expend comparatively large sums before the system was fully proved. The success of radar appears very obvious in retrospect, in a world now technologically alert, but things looked very differently to an administration to which science and technology were mainly background mysteries. It was a tremendous step from the trial of 1935 to the fully operational system of 1939, and very great credit is due to those who brought about the technical development, persuaded the government to pay, and worked out with the Royal Air Force the way in which a radar-based defence system was to be operated. The men primarily responsible were Watson Watt and his associates at Bawdsey, and Tizard and his colleagues in London.

A new technique such as radar can be successfully employed in operations by an armed service only if the officers of that service understand how to use the new technique, and are prepared, if need be, to remodel their previous concepts of organization and tactics. There was in the Royal Air Force in 1935 a cadre of officers of exceptional outlook, who were prepared to work with the scientists with the utmost urgency towards the development of the new air defence system. Tizard was fortunate in having on the one side Watson Watt and his colleagues enthusiastically pressing the concept of radar, and on the other a most receptive body of Royal Air Force officers. Tizard's first contribution was to bring the latent possibilities of this situation to reality. As an administrator who was also a man of science and who had been a serving officer, he saw to the full both the need and the scope for co-operation; and he was exceptionally fitted to lead the balanced development of techniques and operational application that would be necessary if Britain was to have a successful air defence system in the shortest possible time.

The nature of Tizard's contribution may be best brought out by the contrast between the British and German approaches to the development of radar. The appropriate technique was invented in Germany within a very few months of its invention in Britain; and in some respects (but not all) German radar equipment was technically superior to the British in 1939. In Germany, however, there was very little of the interplay between the scientists and the serving officers that developed in Britain under Tizard's stimulation, and the German scientists and engineers developed radar equipments that were primarily measuring instruments in which more attention was paid to absolute accuracy than to operational value. These equipments were taken over by the German Services, who seemed to evolve their method of employment almost independently of the scientists who had developed them. Thus, there appears to have been no scientist in Germany who fully understood the needs of the German Air Force, and no officer in that Service who appreciated what science could reasonably be asked to do for it. Radar, with its long range, was regarded by the German Air Force up to 1941 as a means of economizing in visual Observer Corps posts, and the radar stations were grafted on to the Observer Corps organization. In Britain, by contrast, it was realized that the main difficulty in defence against air attack before the invention of radar was the need to fly standing patrols of fighters. By the time visual observer posts had reported the raiders, it would be too late to send up fighters from the ground. Radar, with its longer warning of the approach of the raiders, did give enough time for fighters to be sent up from the ground to intercept them, and for these fighters to be sent to the right region for interception. By removing the dual uncertainties of time and place of attack, radar allowed a great economy in fighters, and this was a much more important economy than that in observer posts.

Air defence thinking was thus much more fundamental in Britain under Tizard than it was in Germany under the officers of the German Air Force. Once radar was seen to be the key to the defence system, it was logical to base the whole system on it. A specially rapid reporting network had to be set up, so that as little as possible of the valuable warning time should be lost; and radio communications with the fighters in the air had to be brought up to a new standard. Moreover, the technique of interception based upon radar plots had to be worked out. This provided one of the best examples of Tizard's foresight and stimulation.

In 1936, before the radar defence chain existed, Tizard persuaded the Royal Air Force to assume that the chain was in being, and that it would provide the appropriate plots of a raiding aircraft. The situation could be simulated by flying an aircraft along a predetermined raiding course, and feeding its 'plots' to the defence system. It could then be evaluated by practical trials whether the data were sufficient to permit successful interception, and whether one interception technique was more suitable than another. The trials were conducted in 1936 from Biggin Hill, and the serving officers involved still retain warm memories of Tizard's personal impact while the interceptions were being carried out. He would talk to anyone, civilian or serving officer, of whatever rank, if he could thereby gain contact with first-hand experience, or find a new suggestion for solving a problem. As a result of the trials, Britain had not only the radar equipment on which to base her air defence system in 1939; she also had the whole system, manned by a body of men who knew how to use it.

The interplay between science and the services, which Tizard so stimulated and fostered, was vital to the survival of Britain in 1940. It would often happen that the serving officer would express a requirement that could not be met exactly by the scientist. In Britain, the latter was able—by virtue of the co-operation that had developed—to point out that if the serving officer could modify his requirements, an apparatus might be produced which would serve the purpose. Moreover, the scientist might conceive of an apparatus for which the serving officer had previously felt no need, but which could offer a completely new possibility in warfare. Many ideas were, of course, tried and failed; but the prospects of a fair trial were better under Tizard than they had ever been before, and the successful exploitation of such ideas as radar in its many forms, and also the jet engine, is evidence enough of his leadership and judgement.

In 1937 the Committee for the Scientific Survey of Air Offence was formed along the same lines as its Defence counterpart, again with Tizard as Chairman. Tizard himself recorded that, in comparison with the C.S.S.A.D., 'it did not meet with such an enthusiastic welcome from the Air Force. As a result its influence before the war was small.' As already mentioned, at this time Tizard must have been almost fully occupied with his Chairmanship of the C.S.S.A.D. and the A.R.C., and with the Rectorship of Imperial College. He had insufficient effort left over to reorientate the minds of those officers responsible for offensive operations; this was a more difficult task than the defensive one had been, partly because offensive thinking was repugnant to the country as a whole, and partly because the Royal Air Force did not realize its navigational shortcomings, which showed up only after a year of war experience. If Tizard had been free of all other commitments, he might have been able to stimulate the scientific development of air offence almost as much as he had done for defence; but the comparison shows that even Tizard could be effective only when an armed service was prepared to face its own weakness.

If much of this Memoir has been concerned with the co-operation fostered by Tizard, it is because this was the greatest lesson to be learnt from his example, and he would have welcomed this opportunity of underlining it. In his own words:

> 'The first time, I believe, that scientists were ever called in to study the needs of the Service as distinct from their wants, was in 1935, and then only as a last resort. The Air Staff were convinced of the inadequacy of existing methods and equipment to defeat air attack on Great Britain, and a committee was established for the scientific survey of air defence. I want to emphasize that this committee, although it consisted on paper only of scientists, was in fact from the first a committee of scientists and serving officers, working together . . .
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> 'When I went to Washington in 1940 I found that radar had been independently invented in America about the same time as it had been invented in England. We were, however, a very long way ahead in its practical applications to war. The reason for this was that scientists and serving officers had combined before the War to study its tactical uses. This is the great lesson of the last war.'

While the fostering of radar in its many forms and applications was the outstanding contribution of the Tizard Committee, the Committee initiated several other developments in the British defence system. Operational research grew out of the partnership with the Royal Air Force over radar, and afterwards spread to the application of scientific method to all forms of military activity. Discussions in the Committee also revealed how little was known about the German applications of science and technology to warfare. Tizard thereupon persuaded the intelligence organization, a naturally secretive body, to accept a scientist to work with it. This was the start of scientific intelligence, which was afterwards taken up by other major powers: and if scientific intelligence played a useful part in 1940 and later, the credit for its primary stimulation is Tizard's.

The Tizard Committee had been strengthened in 1936 by the addition of Professor E. V. Appleton, F.R.S., to its membership, and in 1939 of T. R. Merton, F.R.S. In May 1939, uranium oxide was discussed in connexion with the possibility of making a nuclear bomb, and Tizard tried semi-officially to obtain for the British Government an option on each ton of ore extracted from the Shinkolobwe mine in Katanga. While Tizard seems never to have believed that a nuclear bomb would have any serious influence on the 1939-1945 war (and even in early 1945 he could hardly believe that such a bomb would ever be feasible) he gave good support to the early evaluation of its possibilities. This was characteristic of his approach: he was prepared to look at any suggestion, and to keep his mind open until proof or disproof was forthcoming. With infra-red detection, for example, despite his initial doubts of its success, he had recommended in 1937, 'that the experiments now being conducted at Oxford on infra-red radiation should continue in the hope that they will have an application to air defence other than the detection of aircraft from the air'. Since these experiments were taking place in Lindemann's laboratory, the recommendation speaks for Tizard's open-minded approach.

The return of Winston Churchill to the Admiralty in 1939, bringing with him Lindemann as his adviser on scientific and economic matters, foreshadowed difficulties for Tizard and his Committee. Tizard himself accepted an appointment as Scientific Adviser to the Chief of Air Staff, and friends tried to close the breach between him and Lindemann. Tizard was anxious for a rapprochement, and Brigadier Charles Lindemann actually persuaded the two men to shake hands in the Athenaeum. But Lindemann could not forget his earlier suspicions, and Tizard found his position in Whitehall increasingly difficult after Churchill became Prime Minister.

Tizard contemplated resignation in June 1940, but this step was allayed by developments in Washington. Some time before, Tizard had suggested that scientific co-operation with the United States should be fostered by attaching a scientific adviser to the British Ambassador in Washington, Lord Lothian; the latter had already suggested to the British Government that an interchange of scientific information and service experience should take place between the two countries. A. V. Hill therefore went out as an Attaché to Washington in May 1940 but, finding that he had an insufficient mandate to release information, he came back to London to press for stronger action. As a result, Tizard was asked to head a mission; Tizard wrote his own terms of reference, which were approved by the Prime Minister:

> 'To tell them what they want to know, to give all assistance I can on behalf of the British Government to enable the armed forces of the U.S.A. to reach the highest level of technical efficiency.'

Other members of the Mission included J. D. Cockcroft, F.R.S., and R. H. Fowler, F.R.S. They carried with them many technical secrets, including the 10 cm magnetron and the proximity fuse. They also carried, perhaps without specifically realizing it, the great secret of co-operation between science and the services. The impact in September 1940 of the Tizard Mission on military and scientific thinking in the United States was immense. The revelation of so many secrets without guarantee of an adequate return was an act of balanced courage, in which Tizard's judgement must have been a predominant factor. Thereafter there remained, except perhaps in the field of nuclear energy, an exemplary co-operation between the two countries on scientific matters throughout the war.

Tizard told a story of one experience on this Mission, concerning two of the many inventors who besieged him with suggestions to help Britain to win the war. These two inventors claimed that they had an aircraft that carried a lethal ray which could destroy a town, and they were prepared to sell the secret to Britain for one million pounds. Tizard asked them if they could immediately provide a demonstration on a not-too-difficult target, say Brest. They replied that they could not do so, since they kept the aircraft hidden in Alaska for security. Finally, they agreed that the destruction of Brest should be possible in a week's time, but only if Tizard would first provide a million pounds. Tizard thereupon wrote on Embassy paper, 'In the event of Messrs. A. and B. destroying Brest within ten days, the British Government undertakes to pay them one million pounds free of tax.' The inventors were not seen again, but there was some disquiet at the Embassy because Treasury sanction had not been obtained in advance.

Tizard returned to London early in October 1940, but he played a decreasing part in operational matters. He moved to the Ministry of Aircraft Production towards the end of 1940, and became responsible for research and development there. When this Ministry was reorganized in July 1941, he held a roving commission in regard to scientific research, and represented the M.A.P. as a member of the Air Council. Here he found, as is not uncommon even at the highest level, that much of the time was spent on minor administrative matters; he related that at one meeting of the Council the main business was to inspect competing designs for W.A.A.F. underwear. One development inspired by him was the sending of a mission to North Africa under the leadership of S. Zuckermann (later Sir Solly Zuckermann, F.R.S.) to assess the results achieved by British weapons. In this period Tizard fulfilled the functions of an 'Inspector General' for science, in a manner very similar to that sometimes carried out by retired Commanders-in-Chief. The Chief of Air Staff would call on him to undertake a specific investigation—for example, into the merits of unifying all the scientists in the Air Ministry under a single control, or into the measures to be taken to offset the premature loss of our radio-navigational equipment to the Germans.

In general, Tizard's advice was sought, respected and acted upon by the Air Staff much as Smuts's was by the War Cabinet. His tasks were not, however, enough to satisfy him, particularly when his advice on major issues was likely to be outweighed by that of Lindemann. A controversy regarding bombing effort sprang up between them early in 1942. The importance of this particular controversy can easily be exaggerated; it was not simply a matter of Lindemann wanting sadistically to bomb German towns, and Tizard opposing it on humane grounds, with Lindemann losing the scientific argument but winning the political battle and thereby determining Cabinet policy. Tizard did not, he wrote, fundamentally disagree with the bombing policy, but he thought that it could not be carried out effectively with the forces likely to be at the disposal of the Royal Air Force; and he was very concerned that in concentrating the bombing effort against towns, we should fail to use sufficient aircraft against enemy ships of war, and that we should thus lose command of the seas. Tizard's view appears in retrospect to have been more balanced than Lindemann's, but it would be wrong to assume that either view was a decisive factor by itself in the bombing policy. The issue, which was basically between offensive and defensive uses of bombing resources, was fought out between many protagonists, political, military and scientific. Lindemann and Tizard could both express their arguments unusually well, and could give quantitative weight to some of the factors; these qualities brought them to the opposing forefronts of the controversy. This is not the place to argue out the full merits of the two sides; it is sufficient to record that as far as Tizard was concerned, his judgement was sound, and that any impatience that he may have felt regarding the neglect of his advice is understandable.

Shortly after this dispute, Tizard left his M.A.P. appointment to become President of Magdalen. There is some evidence that Lindemann (by that time Lord Cherwell) aided him in the election. The four years for which he held the Presidency (1942-1946) were, of course, quite abnormal ones for the College. He initiated several administrative reforms, including a reorganization of the Bursary, a revision of the relations between the College and its Schools at Brackley and at Oxford, and an expansion in the number of undergraduates. He was brilliant in his concepts of what the College should be but, having held posts of great authority, he found it rather difficult to pilot controversial business through a highly democratic body of Fellows. A colleague has noted that he would drop his own ideas at any sign of opposition; this same quality had at times been evident in his defence work.

Tizard continued to be called in on various matters of official business. He went to Australia in 1943 to advise the Australian Government over a wide range of military and civil scientific research organization. Early in 1945, he was Chairman of a Chiefs of Staff Committee on the future of Weapons, and in 1946 he was re-called to full-time Government service, there being now no obstacle since Cherwell had returned to Oxford in 1945.

As science had come to play an increasing part in the war, it had needed a forum in which all the scientific effort could be co-ordinated. At first this was effected by the Deputy Chiefs of Staff Committee, which was attended by these officers and by senior scientists associated with the Service Ministries. Early in 1945 a committee was formed under Cherwell's chairmanship to take over the scientific functions hitherto carried out by the Deputy Chiefs of Staff Committee, but Cherwell ceased to be Chairman after the General Election of that year. In the reorganization that followed the separation of the Ministry of Defence from the Premiership, the successor to Cherwell's committee was the Defence Research Policy Committee. At the same time, a roughly parallel committee for civil aspects of science, the Advisory Council on Scientific Policy, was set up by the Government. Tizard was invited to the Ministry of Defence, and to become Chairman of both the D.R.P.C. and the A.C.S.P.

Tizard was easily the best man for the new post, which he had to steer through a peculiarly difficult time. The Ministry of Defence was uncertain of its philosophy and of its position with respect to the three Services. The whole defence system was re-orientating itself towards the new technological situation created by such developments as nuclear energy and rockets, with Britain having to accept a more subdued part in the world's affairs and carrying through a social upheaval at home, in the face of a series of crises typified by the Berlin blockade. This was a far more complex situation than anything that had had to be faced before or during World War II, and without the ready test that war provides of whether a particular decision is right or wrong. It is therefore not easy to assess even now, after a lapse of ten years, the success either of a policy, or of an individual's personal decisions.

One handicap that Tizard faced was that the D.R.P.C. was not allowed to concern itself with the development of nuclear weapons. This restriction, which persisted for some years after the first Russian nuclear explosion had been reported, was a discouragement to conspective thinking about national defence. The dichotomy between 'nuclear' and 'non-nuclear' aspects of defence led to much frustration of the latter because of the abnormal priority and secrecy given to the former. Tizard may well have found his position unsatisfactory on this account.

Tizard gave the D.R.P.C. the best possible start in these difficult circumstances. His prestige with the services was immense, he was a skilful chairman, and he was a good judge of men. He was particularly well matched to the task of presiding over the periodic meetings of the Commonwealth Advisory Committee on Defence Science, and he much enjoyed the social contacts which these meetings afforded with scientists and serving officers from all over the Commonwealth. The organization of the D.R.P.C. has survived substantially in the form in which he left it on retirement in 1952, and the co-operation that he so effectively fostered between science and the armed services remains as an inspiring example.

His main achievements as Chairman of the A.C.S.P. were to convince the Government of the value of the advice that the Council could provide, and to overcome the suspicion with which the Council was at first viewed by some Government Departments and Research Councils. He insisted on science taking its proper place in Government Departments, and he developed a policy, which is still accepted, regarding the relations between the scientific work of the Departments and that of the Research Councils. In other hands, the A.C.S.P. might well have foundered; in Tizard's it became a workable entity occupying an important place in Government.

He retired from the Advisory Council on Scientific Policy in 1952, having given in the six years of his chairmanship increasing attention to the organization and application of science in peace. While at the Ministry of Defence he undertook several major visits to Commonwealth countries and the United States, and his visits were as much concerned with the broader aspects of science as they were with defence. He had had much to do with these aspects in his work at the D.S.I.R., and he evidently felt them particularly attractive and rewarding in the post-war years.

Tizard's outstanding service to Britain and to the Dominions was recognized by his advancement in 1937 to K.C.B. and in 1949 to G.C.B. From the United States, where his fame was as high as in his own country, he received in 1947 the Medal for Merit.

During the period 1933 to 1956 Honorary Degrees were conferred on him by a number of Universities, he was elected to Honorary Fellowships of Colleges in Oxford and in London, and received the Gold Medals of three Societies.

[*End of Professor R. V. Jones's Contribution*]

As Foreign Secretary of the Royal Society from 1940 to 1945, Tizard was one of the closely knit group of Officers who guided and administered the policy of the Society through the war years. With Sir Henry Dale, Sir Thomas Merton, A. V. Hill and Sir Alfred Egerton in those days when meetings were at a minimum, decisions and action had to be taken rapidly and firmly, and frequently one or other of the Officers was abroad on a special mission, the functions of a Foreign Secretary were less called upon and duties were apt to be interchanged. Nevertheless in the field of foreign relations the exchange of information on scientific and technical problems primarily arising out of the war brought to London scientific representatives from the various Dominions as well as from the United States and allied countries with whom the international connexions of the Royal Society were maintained and strengthened. Under Tizard's chairmanship the International Relations Committee in 1943 considered the whole question of the post-war organization of international scientific co-operation with special reference to the future of the international unions and the Society has not looked back on its resolve to give the fullest support to the International Council of Scientific Unions and the federated bodies. During his Foreign Secretaryship the Council set up a Cultural Relations Committee with Tizard as chairman to advise on the development of cultural relations with foreign countries with particular reference to the post-war period of reconstruction and, following recommendations of this committee, visits were later made by Royal Society representatives to the national academies of certain countries then newly liberated from alien occupation, and means were explored for assisting the rehabilitation of science and research. One of the means adopted was the provision of scientific apparatus through the Horace Darwin Fund for research institutions abroad.

In 1945 as a result of representation by the Council to the Secretary of State for Air and the First Lord of the Admiralty that aircraft could be profitably used to promote general scientific research after the war, a new joint committee, called the Advisory Committee on Airborne Research Facilities was set up 'to consider proposals for the use of naval and R.A.F. aircraft and facilities for assisting scientific research and to make recommendations to the Council of the Royal Society, the Lords Commissioners of the Admiralty and the Air Council'. It consisted of representatives nominated by the Council, the Admiralty, the Air Ministry and the Ministry of Aircraft Production, with Tizard as its first chairman. This committee did useful work and continued until its functions were merged with those of the present Defence Services Research Facilities Committee.

In March 1952 Tizard retired finally from the public service. From letters which he wrote to Professor A. V. Hill, F.R.S., it is clear that he was burdened by what he describes as 'my perennial attacks of fever—a curse in my life.' These did not deter him from visiting Pakistan and India in this year, as a representative of the British Association, in company with Professor Hill. On his return, he developed a heavy cold which gave him great trouble and he began 'to think that I am a broken down old gent, mentally and physically'. Characteristically, the same letter goes on to say 'I am now busy writing a Messel Lecture for the Society of Chemical Industry.' In August 1952 he said: 'Three half-fainting fits recently have convinced me that I have been stretched beyond the elastic limit in recent years. I have been trying to hide this from myself but can do so no longer. The doctor says, however, that I should be a reasonable member of society after a rest.'

He took a rest, and was soon active again. He became a Director of several chemical companies: Glaxo Laboratories, Albright & Wilson, Blair Knox, Marchon Products and Solway Chemicals. The income from these relieved some of the anxiety about money from which he was seldom free.

He visited Canada in September 1954 as guest of the Defence Research Board and again in 1956 as guest of the National Research Council. His last voyage abroad was to the U.S.A. in 1957, when he attended the 50th Anniversary of the Founding of the State of Oklahoma.

The bibliography which concludes this memoir gives the dates and titles not only of papers describing Tizard's research work, but also of a number of papers of a different kind, which in his case give a far better impression of the man and his work. From one or two, comparatively long extracts have already been given. There is not space to do justice to the content of all his published lectures and addresses, but all may be read with advantage, and with pleasure. In 'Science and democracy' (1951) he took the opportunity provided by an American audience to let his mind run free over an imaginary future, when a world-Government was striving, without much success, to remedy some of the weaknesses of the world as we know it. He asks:

'Why are we scientists and technologists so anxious about the social implications of our work? There are two main reasons. The first is the haunting, but perhaps unjustified fear that, if ever war comes again on a large scale, the misapplications of the powers of science will be so frightful as altogether to destroy civilization as we know it. The second is that while the application of scientific knowledge has brought such great benefits to mankind, it has not brought a fraction of what, in our opinion, should have been possible.'

His Messel Memorial Lecture before the Society of Chemical Industry in 1952, 'The strategy of science', contains the most valuable and provocative of his surveys of the place of science in the nation's life. He concludes that:

'. . . the springs of enterprise are running down, and unless industry is able and allowed to increase its capital expenditure, and does it wisely, it is idle for scientists to complain that their discoveries are exploited elsewhere. At present the responsibility for deciding whether any branch of industry shall spend anything on new ventures rests largely with civil servants in government departments. How can they exercise it properly? It is not their fault; it is their misfortune.'

and that:

'. . . just as success in pure science rests in the choice of the right problem to attack, so does success in industrial adventure. When money resources are limited it becomes all the more necessary to concentrate the scientific resources. Strategy is all important.'

Finally, he agrees with Benjamin Franklin's confession:

'For having lived long, I have experienced many instances of being obliged by better information or fuller consideration to change my opinion even on important subjects, which I once thought right but found to be otherwise. It is therefore that the older I grow the more apt I am to doubt my own judgment, and to pay more respect to the judgment of others.'

Nevertheless, he holds to certain convictions:

'It is more important now to strengthen our technology than to expand our science; more important to do things than to write about how they might be done. Science is not enough. We haven't any money, so we've got to think.'

The last in this category, 'A scientist in and out of the Civil Service', the Haldane Memorial Lecture delivered at Birkbeck College in 1955, is in the nature of a pocket autobiography. It suggests an answer to the question —Why did Tizard leave the D.S.I.R. in 1929 for the Imperial College? He was convinced that neither research under the D.S.I.R. nor research in industry could produce results justifying the expenditure unless we could ensure a large increase in the right kind of men, technologists as well as men of science. He spoke eloquently on the need to ensure that research workers in both fields should live and work cheek by jowl with those in whose interests they were working.

In the last few years of his life (1955 to 1959) he occupied himself with trying to get his papers in order, and with completing the 'personal record' for the Society, which he signed in May 1959. This record can fairly be described as faultless both in length and in choice of material. It is significant that he made no mention of Lindemann, except the bare facts, related above by Professor R. V. Jones, associated with the C.S.S.A.D. in 1935/1936.

It concludes with two short paragraphs addressed:

'To the writer of my Obituary Notice'

'I am sorry for you: but still you have these notes to help you. I had nothing of the kind to help me when I was engaged on the laborious task of writing the obituaries for which I was responsible. I even had to consult Parish registers to make certain of some dates!

'I am now engaged in writing the story of my life for the private information of my family. It has not got very far yet: but if it gets finished before my death, you will be able to get more details, if you need them, from my son, Dr J. P. M. Tizard.'

The story, which is rather a collection of memories of the first half of a long life than a serious autobiography, covered only the years to about 1927, though in places it contained references to later events. It adds little to the 'personal record' concerning the events that made up his scientific and professional life, but it brings into relief the years up to 1904. When Tizard went to Westminster in 1900 his inborn talent for mathematics left him with time to feel the influence of the arts at a school which was predominantly classical. When he was in the Modern Sixth, he challenged the Headmaster: 'Why are we taught no English on the modern side?' A chosen few were sent to John Sargeaunt—to whom he was ever after grateful for informing and encouraging his love of his own language and its literature.

And 'Westminster was a splendid school for boys of sensibility.' Near-by, in the Abbey and the Houses of Parliament, he watched the pageant of history. Queen Victoria's funeral passed: Edward VII was crowned: from the Strangers' Gallery the privileged King's Scholar observed the workings of the machinery of democracy. No wonder the magic of Oxford had its way with him— 'the beauty of the place, particularly Magdalen, sunk into me'. It is in keeping with his genius for surprising us that all his life he was a strong advocate, not of courses in arts for men of science—but of courses in science for men of the arts and the humanities.

He delivered the Rutherford Memorial Lecture before the Chemical Society on 29 March 1939, certainly the most quotable of all his addresses. But it was of words he had used of Sir William McCormick that I thought, at the Memorial Service in King Henry VII's Chapel in Westminster Abbey on 19 November 1959:

'His work for science was done before he died, as well and as thoroughly as any man's work was ever done.'

Tizard died of a cerebral haemorrhage, after an illness which lasted only a few hours, at his home 'Keston', Hill Head, Fareham, on 9 October 1959.

I have already mentioned on p. 331 my indebtedness to Professor R. V. Jones for his help in dealing with what is possibly the most significant part of Tizard's life. I wish particularly to acknowledge his help in many discussions we have had about points where information was conflicting or even lacking.

Professor Jones wishes to acknowledge the generous help he has received from those whose names follow:

Sir Edward Appleton, F.R.S.
Sir John Cockcroft, F.R.S.
Mr Hayne Constant, F.R.S.
Dr F. A. Freeth, F.R.S.
Mr R. W. Gandy
Dr J. H. E. Griffiths, O.B.E.
Mr L. A. Jackets, M.B.E.
Sir Ben Lockspeiser, F.R.S.
Sir Thomas Merton, F.R.S.
Air Chief Marshal Sir George Mills
Sir Harold Roxbee Cox
Dr A. P. Rowe, C.B.E.
Lord Tedder, G.C.B.
Sir Alexander Todd, F.R.S.
Sir Robert Watson-Watt, F.R.S.
Mr A. J. D. Woods, C.B.E.

I have been greatly helped by discussions with Lord Adrian, F.R.S., Sir Sydney Barratt, Sir Geoffrey Taylor, F.R.S., and Sir George Thomson, F.R.S., all life-long friends of Tizard.

I have to thank also all those mentioned below who have generously given me their help in the form either of letters or discussions, or both. Without these I should not have been able to write what I have throughout attempted, a story of Tizard's life which would do justice, within the space at my disposal, to the man and his work for science and for his country.

I am specially grateful to Sir Harold Hartley, G.C.V.O., F.R.S., not only for giving me freely of his own memories of Tizard over 55 years, but also for allowing me to see and to quote from letters from some of his friends who were also friends and colleagues of Tizard, namely:

Sir Harry Ricardo, F.R.S.
Mr R. E. Threlfall
Sir Patrick Linstead, F.R.S.
Mr G. C. Lowry
Dr H. J. T. Ellingham
Mr G. R. D. Hogg
Dr R. S. Hutton
Dr L. E. Sutton, F.R.S.
Dr D. G. James

To Professor P. M. S. Blackett, F.R.S., I must express my acknowledgement of the help I have derived from his Tizard Memorial Lecture before the Institute of Strategic Studies, 11 February 1960 'Tizard and the science of war' (*Nature, Lond.* **185,** 647, 1960).

Professor A. V. Hill, C.H., F.R.S., kindly lent me certain letters which passed between him and Tizard, which threw much light on the last years of Tizard's life.

I am indebted to Dr D. C. Martin and Miss Phyllis Boatwright of the Royal Society's staff for the notes on Tizard's work as Foreign Secretary of the Society from 1940 to 1945.

Finally, I must thank Dr J. P. M. Tizard and Dr Noble Frankland for allowing me to read and take notes from the unfinished 'Autobiography' upon which Tizard was engaged until well into 1959.

W. S. Farren.

Public Honours

Air Force Cross, 1918
Companion of the Bath, 1927
Knight Commander of the Bath, 1937
Knight Grand Cross of the Bath, 1949
Medal of Merit, U.S.A., 1947

Honorary Degrees

D.C.L. Durham University, 1943
Ll.D. Queensland University, 1943
D.Sc. Leeds University, 1945
Sc.D. Cambridge University, 1946
Ll.D. Edinburgh University, 1946
Ll.D. Sheffield University, 1948
D.Sc. Belfast University, 1949
D.Sc. Reading University, 1951
D.Sc. Manchester University, 1952
D.Sc. University of London, 1956

Honorary Fellowships

Oriel College, Oxford, 1933
Imperial College, London, 1942
Magdalen College, Oxford, 1947
University College, London, 1954

Medals and Awards

Gold Medal of Royal Society of Arts, 1944
Gold Medal of Franklin Institute, 1946
Messel Gold Medal of Soc. Chem. Ind., 1952

Membership of British Scientific Societies

Member of Chemical Society, 1909

Member of British Association, 1911
    President of Section 1, 1934
    President of Association, 1948

Fellow of Royal Society, 1926
    Member of Council, 1933/1934 and 1940/1941
    Foreign Secretary, 1940/1945
    Vice President, 1940/1941 and 1944/1945

Fellow of Royal Aeronautical Society, 1917
    Hon. Fellow, 1951
    Chairman, 1924

Member of Faraday Society 1919
    Life Member from 1953

Fellow of the Royal Society of Arts, 1929
    Vice President, 1929/1932

Founder Fellow, Institute of Physics, 1920

Hon. Fellow, Royal Institute of Chemistry, 1952

Hon. Membership of Institute of Chemists, India, 1938
Hon. Fellow of Australian Institute of Mining and Metallurgy, 1948
Hon. Member of Institution of Mining and Metallurgy, 1951
Hon. Member of Pakistan Assoc. for Advancement of Science.

Foreign Societies

Fellow of Institute of the Aeronautical Sciences, U.S.A., 1932
Foreign Honorary Fellow, 1950
Foreign Member of Deutsche Luftfahrt Forschung, 1937
Hon. Member of Franklin Institute, U.S.A., 1946

Other distinctions

Elected Member of Athenaeum Club, under Rule 2, 1930
President of Science Masters Association, 1934
President of Fourth Empire Mining and Metallurgical Congress, 1949
Trustee of the British Museum, 1937/1959
Pro-Chancellor, Southampton University, 1952
Prime Warden of the Goldsmiths Company, 1955/1956

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1913. (With J. S. Townsend.) The motion of electrons in gases. *Proc. Roy. Soc.* A, **88,** 33.
1917. Methods of measuring aircraft performances. *Aeronautical Society's Reprints,* No. 2.
1920. (With J. R. H. Whiston.) The effect of a change in temperature on the colour changes of methyl orange, etc. *Trans. Chem. Soc.* **117,** 150.
1921. (With A. R. Roeree.) The volumetric estimation of mixtures of acids and of bases, etc. *Trans. Chem. Soc.* **119,** 132.
1921. (With A. G. Marshall.) The determination of aromatic hydrocarbons in mixtures of hydrocarbons. *J. Soc. Chem. Ind.* **40,** 20T.
1921. The causes of detonation in internal combustion engines. *Proc. N.E. Coast Inst. of Engrs Shipbuilders.* May.
1922. (With A. G. Marshall.) Method for the determination of the vapour pressure of hydrocarbon fuels and the estimation of dissolved air. *J. Instn Petroleum Tech.* **8,** 217-223.
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1926. Explosions in petrol engines. *Trans. Faraday Soc.* **22,** 352.
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1911. Revised and largely retranslated 6th German edition of Nernst's *Theoretical chemistry,* for third English edition, pubd. by Macmillans.
1938. Co-Editor: *Science of Petroleum,* O.U.P.

Special Lectures

1929. The Mather Lecture: Science and the new industrial revolution. *J. Textile Inst.* **4.**
1932. First Hinchley Memorial Lecture: Chemical engineering and the aircraft industry. *Trans. Inst. of Chem. Engrg,* **10,** 87.
1934. Presidential Address to the Science Masters Association: Science and the industrial depression. *School. Sci. Rev.,* No. 59. p. 257.
1934. Presidential Address to Section L. of British Association: Science in the universities. *B.A. Reports.*
1938. First Perin Memorial Lecture: Industrial research, present and future. Publ. by the Tata Iron and Steel Co. January.
1939. Rutherford Memorial Lecture to the Chemical Society. Delivered 29 March, Publ. *J. Chem. Soc.* October 1946.
1948. Presidential Address to the British Association: The passing world.
1951. Arthur Dehon Little Memorial Lecture at Mass. Inst. Technology: Science and democracy. Publ. by M.I.T.
1952. Messel Memorial Lecture: The strategy of science. *Chem.* & *Ind.,* p. 788.
1955. Haldane Memorial Lecture: A scientist in and out of the Civil Service. Birkbeck College.

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1931. The British fuel problem: future possibilities. *Chemistry at the Centenary Meeting, Brit. Assoc.*
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Bertram Hopkinson: 5 March 1937.
Lord Rutherford: 16 December 1945.

JHC Whitehead

JOHN HENRY CONSTANTINE WHITEHEAD

1904-1960

Henry Whitehead was at the height of his powers and of his mathematical influence when he died suddenly on 8 May 1960 after a heart-attack. In the last months before his death he had written several important papers, which showed how much he had still to contribute, and how keenly alive he was to the value of the newest work of young contemporaries.

His life was a happy one, with little for the biographer to chronicle. He was born in India on 11 November 1904, the son of the Right Reverend Henry Whitehead, for twenty-three years Bishop of Madras and sometime Fellow of Trinity College, Oxford; and of Isobel Whitehead daughter of Canon Duncan of Calne in Wiltshire. She was one of the early mathematical students of Lady Margaret Hall. There were many clerics and teachers among earlier generations of Whiteheads; the philosopher A. N. Whitehead was Bishop Whitehead's brother.

At the age of one and a half the young Henry was sent back to England and saw little of his parents until their retirement to England in 1920. At Eton, and as a Balliol undergraduate, his high spirits and convivial habits and his many-sided successes at games gave little grounds for expecting him to excel on the academic side. He boxed and played billiards for the University, and just missed winning a blue for cricket. Even after his rather easily won First Classes in both Moderations and Finals, it seemed quite natural to his family, and to his College tutor, that he should go off to the City to start on a financial career. But after little more than a year he broke away and returned to Oxford to do more work in mathematics (1928-1929). There he met Professor Oswald Veblen, on leave from Princeton University. This meeting, and the award of a Commonwealth Fellowship, were events of critical importance in his life, for it was in the next three years, spent at Princeton, that the permanence of his interest in mathematics and the reality of his talent were established beyond doubt. Many friendships were begun during that visit; indeed the declared purpose of the Commonwealth Fund, to promote mutual amity and understanding between the English-speaking peoples, can rarely have been more thoroughly and satisfactorily fulfilled.

In 1932 he returned to Oxford and soon afterwards was elected to a Fellowship at Balliol. In 1934 he married Barbara Smyth, a young concert pianist, and they set up house in St Giles, in the heart of Oxford. There were two sons of the marriage. In their house in Oxford, and later at the Manor Farm in Noke, generations of mathematicians, including Whitehead's own students and many visitors from abroad, enjoyed their delightful hospitality and some serious mathematical conversation.

From 1941 to 1945 he was fully engaged in war-work for various government departments. It was during this time that he was elected a Fellow of the Royal Society (1944).

His election to the Waynflete Chair at Oxford was fortunately timed. With a substantial body of work behind him, including his two big papers on combinatorial homotopy (28) and the Stiefel manifolds (37), he was ready to start building up a school of topology in Oxford. This he set about doing, with the brilliant success that is well known to all mathematicians.

There are few more events to be told. He received many invitations from foreign universities, to deliver lectures or to make prolonged stays. Some he accepted, but on the whole he preferred to stay in Oxford and entertain there his many mathematical friends. He was President of the London Mathematical Society in 1953-1955, but held few other offices. Through a mixture of good fortune and design his single-minded devotion to mathematics was never seriously interrupted, except by the four years of war work.

His mathematical life was spent in a series of long and strenuous campaigns, in which he attacked and wore down the resistance of large and deep problems. The longest of all, which lasted on and off for fifteen years, was the characterization of the homotopy-type of complexes and spaces, of which the first stage, the search for a combinatorial description, was completely successful, and the second stage very nearly so. Among the by-products of this campaign were some of the most valuable tools of modern algebraic and combinatorial topology: the homotopy sequence of a complex and subcomplex, first introduced at the end of paper (37); the 'Whitehead product'; the 'regular neighbourhoods', which have been found to be of fundamental importance in the brilliant advances in combinatorial topology that have been made in the last two years. This revival of geometrical topology made his early death doubly tragic, since there were many signs that it had started him afresh on that blend of geometrical and algebraical thinking which came more naturally to him than the rather rigid formality of his work in the later nineteen fifties.

As a mathematical writer, Whitehead had little time and not much taste for elegance. He gave his readers a rough ride, seldom favouring them with those informal 'Leitfaden' which help to smooth the way. Even the titles of some of his later papers seem designed to discourage the light-minded. The style of his mathematical letters, though terse, was much easier, through the use of just those colloquial touches and geometrical 'cribs' that were missing, or even deliberately taken out, from the published papers. But his preferred medium was mathematical talk, for which he never lost his appetite, and in which he excelled. If he expected concentrated attention, he gave it in full measure himself. At the end of one of these leisurely and searching conversations he would understand one's story thoroughly, and if it had survived without mishap one could feel sure that it was right.

The immediate attractiveness of Henry Whitehead's manner, and his tremendous high spirits, would not alone have brought him the affection of so many mathematicians all over the world if there had not been behind them his devotion to mathematics, and his readiness to interest himself deeply in other people's problems. His unusually acute perception of the thoughts and moods of others enabled him to accept and enjoy every kind of human behaviour, except perhaps a certain kind of conscious dignity, which he heartily disliked. His own easy informality, though it might be disconcerting to hosts and hostesses on formal occasions, when his spirits rose, and he might even break into song, enabled him to talk about mathematics on equal terms with one and all. It did not conceal the natural authority which in his later years was given to him by his mathematical achievement and by his profound belief in the importance of winning the mathematical sieges and battles of which so much of his work seemed to consist; and it made easy of access the generous help which he would give not only to his pupils, but to anyone with a mathematical problem to discuss and get right.

His friends will not soon be consoled for their unexpected loss.

Scientific Work

(This section contains an account of Whitehead's major mathematical enterprises rather than a review of his complete works, which will soon be available in collected form. I am grateful to Dr M. G. Barratt for much material on the later work in algebraic topology.)

When Whitehead reached Princeton in 1929 the 'geometry of paths', based on the theory of linear connexions and their generalizations, was already nearing the end of its purely formal development. He wrote a few papers on the formal side of the theory (1 to 5, 8) but soon turned to more geometrical topics. The first outcome was the joint paper (6) and book (7) with Oswald Veblen on the foundations of differential geometry. Here for the first time an exact definition of a differentiable manifold was given, in the form of 'axioms for differential geometry'. Although somewhat difficult to read now, owing to changes in terminology and to the mixing in of the topology, they contain essentially the definition of a differentiable manifold as a Hausdorff space with a system of coverings by neighbourhoods, each with an associated homeomorphism, ψ_i, on to the closed unit ball in R^n. The *class* of the differentiable structure is the highest order of existing continuous partial derivatives of the maps $\psi_i\psi_j^{-1}$ of subsets of R^n into R^n, in so far as these maps exist. Independence proofs of the axioms were provided in the paper (6), notably an example to demonstrate the rather surprising fact that a locally euclidean space can fail to be a Hausdorff space.

Under the influence of Marston Morse Whitehead now turned to problems of differential geometry in the large. In (9, 10, 11) he showed that simple convex regions of finite extent exist in path-geometries, i.e. regions in which any two points are joined by one and only one path which does not leave the region. The climax of his work in differential geometry was the paper (17, 1935) on the geometry of geodesics (paths of minimal length) in an analytic manifold, F, with a Finsler metric. He determined in great detail the properties of the locus of characteristic points of a given point O, i.e. of the points at which the natural projection, p, of the tangent plane, M, at O on to the space F (straight lines through O on to geodesics) fails to be (1,1); and he established the existence of an open n-cell $\mathcal{E}$ in M such that $p(\bar{\mathcal{E}}) = F$ and $p|\mathcal{E}$ is a homeomorphism.

This paper, his last before turning away to topology, has remained one of the important pioneering works on differential geometry in the large. It is the first of his works in which his quality as a mathematician can be clearly discerned.

After two isolated dips into topology (12, and 15 with S. Lefschetz) Whitehead plunged into the midst of the fray with a proof (16, 1934) of the Poincaré hypothesis (that a simply-connected closed* 3-manifold is a topological 3-sphere). The proof contained an error, and the truth of the hypothesis is still an open question (for 3-spaces). But it was the reading and experimentation for the attempted proof, and for his subsequent counter-example for open 3-manifolds, that committed him to topology for the rest of his life.

The counter-example (20, 1935) was based on a geometrical configuration of two polygons which has become so familiar through related examples made by himself and others (notably Bing and his school, and Mazur, 1961) that it seems right to reproduce it here.

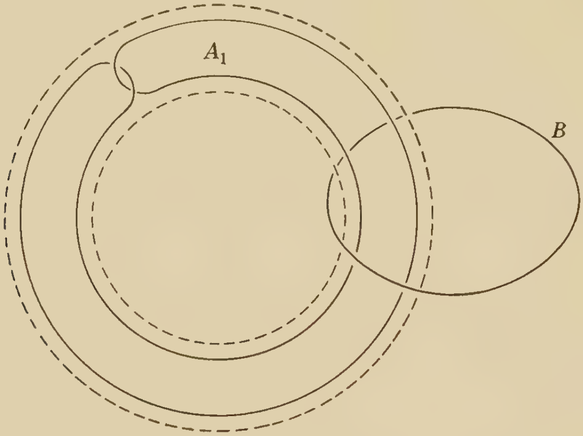

FIGURE 1.

The curve A_1 is clearly null-homotopic (shrinkable to a point, self-intersections allowed) in the open set $S^3 - B$. Moreover, as experiment will show, the positions of A_1 and B can be interchanged by a deformation without self-intersections; and so B is null-homotopic in S^3-A_1. But the polygons cannot be pulled away from each other; more precisely, neither is contained in a 3-cell not meeting the other. Now A_1 is embedded in a solid tube not meeting B (indicated by the dotted lines). If A_1 is itself regarded as a thin solid tube, a 'doubled' tube, A_2 can be similarly embedded in it; and A_3, A_4 can be similarly defined. If $X = \bigcap A_m$, the polygon B and, similarly, any other polygon in S^3-X is null-homotopic in S^3-X, i.e. S^3-X is simply connected; and it is easily seen that $H_2(S^3-X) = 0$. But the compact set B is not contained in any 3-cell in the set S^3-X, which cannot therefore be homeomorphic to R^3.*

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* Compact and without boundary.

In the course of this work he became thoroughly familiar with two varieties of combinatorial topology, at that time scarcely connected with each other, which were to be the basis of his work for the next few years. In some studies on knots and linkages (26, 29, 30) arising out of 'the' linkage (figure 1) he became acquainted with the Reidemeister theory of homotopy based on the group-ring for a complex. From this sprang the important papers (33, 34) which will be discussed below.

The other domain which he now entered was the strictly combinatorial kind of topology which had been developed by J. W. Alexander and myself in the years 1925 to 1932. The subject of these theories is the *skeleton complex*, which is a set, V, with a designated set of finite subsets, called *simplexes*, satisfying the conditions that the simplexes cover V, and that all the subsets (faces) of a simplex are simplexes. Two complexes are *combinatorially equivalent*, $K_1 \longleftrightarrow K_2$, if one can be changed into the other by a succession of allowed transformations, or *moves*, of certain prescribed kinds.

A skeleton-complex can be 'realized' as a Euclidean complex by taking as its vertices points of R^k in general position (k being sufficiently large) and replacing each skeleton simplex by the euclidean simplex with the same vertices (i.e. their convex cover). The *locus* $|K|$, of K is then the point-set union of the euclidean simplexes. The aim of such theories is to choose the allowed transformations so that the relation $K_1 \longleftrightarrow K_2$ corresponds as closely as possible to homeomorphism between $|K_1|$ and $|K_2|$. It is now known that a polyhedron which is not a manifold can have two triangulations which are combinatorially inequivalent, under the moves now generally adopted. For manifolds the question (the so-called *Hauptvermutung*) is still open.

Since the problems of homeomorphism appeared at that time to be quite intractable, Hurewicz had in 1935 begun a general change over to an attack on the more hopeful problems of *homotopy-equivalence* ($X \simeq Y$). This he defined to mean that continuous maps $f: X \to Y$ and $g: Y \to X$ exist such that $gf: X \to X$ and $fg: Y \to Y$ are homotopic to the identical maps in X and Y respectively. The simplest kind of homotopy-equivalence is *retraction by deformation* of X on to a subset, Y, of itself, i.e. a map $f\colon X \times I \to X$ such that $f(x,0) = x$ and $f(x,1) \in Y$ for all $x \in X$, and $f(y,t) = y$ for all $y \in Y$ and $0 \leqslant t \leqslant 1$.

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* In this brief outline distinctions between combinatorial and point-set equivalence are suppressed. The full result, as here stated, was derived in paper (25) from Whitehead's combinatorial theorem.

Whitehead now had the brilliant idea of attacking the theory of homotopy-equivalence by the strictly combinatorial method of allowed transformations. This was the subject of his massive paper (28, 1939), 'Simplicial spaces, nuclei and *m*-groups', one of those on which his reputation as a mathematician will surely rest. As allowed moves he took in the first place the addition of a new simplex, $a\sigma^k$ to a complex K,

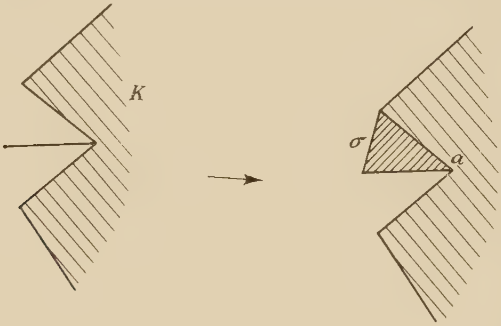

FIGURE 2.

when $a\dot{\sigma}^k$ is a subcomplex of K but $\sigma^k \notin K$.* The transformation $K \to K \cup a\sigma^k$ is then an *elementary expansion*, and $K \cup a\sigma^k \to K$, also allowed, an *elementary contraction*. An *expansion* is a finite series of elementary expansions, and similarly for contractions. A succession of moves of either type is a *formal deformation*, $K_1 \underset{D}{=} K_2$, and two complexes so related have the *same nucleus*.

It is intuitively obvious, and easily proved, that if K_1 is contractible to K_2, $|K_2|$ is a deformation retract of $|K_1|$. It follows that if $K_1 \underset{D}{=} K_2$, $|K_1| \simeq |K_2|$. Simple examples suggest that this implication might be reversible. Whitehead posed this in (28) as a question he was unable to answer, and he therefore introduced another pair of moves, with the help of which he could complete the combinatorial characterization of homotopy type. (This step was later justified when he showed, in (34, 1941), that the answer to his question is 'no': $|K_1| \simeq |K_2|$ does not imply $K_1 \underset{D}{=} K_2$.)

The two further allowed moves were the addition to K, or removal from it, of a simplex, σ^k, of dimension, k, exceeding a fixed number m, when the entire boundary $\dot{\sigma}^k$ is in K. If $K_1 \to K_2$ by a series of moves of any of the four types, K_1 and K_2 have some *m-group*. By means of the *mapping-cylinder*, a device which he first introduced in this paper, he succeeded in giving a thoroughly geometrical treatment of the rather abstract relation of homotopy-equivalence, and he achieved his first main purpose by proving that

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* The join $\sigma^h\tau^k$ of two distinct skeleton simplexes, σ^h and τ^k, is the $(h+k+1)$-simplex $\sigma^h \cup \tau^k$; the join KL of two complexes without common vertices is the set of simplexes $\sigma\tau$ $(\sigma \in K, \tau \in L)$. $\dot{\sigma}$ is the set of all faces of σ except σ itself.

a necessary and sufficient condition for $|K_1| \simeq |K_2|$ *is that* K_1 *and* K_2 *have the same m-group for every m.*

(The somewhat bizarre names 'nucleus' and 'm-groups' for abstract equivalence classes he later (50, 1950) changed to *simple homotopy type* and *m-type* respectively, generalizing the two definitions at the same time. The more familiar name 'simple homotopy type', will be used in this memoir; but as 'm-type' was still later changed to '$(m-1)$-type' it seems best to keep to the unambiguous 'm-group'.)

The second part of this paper contains theorems the importance of which has only recently been fully appreciated, though their use by Mazur, Stallings, Zeeman and others in problems related to the general field of the Poincaré hypothesis. They were the first of those theorems which show that certain dimension-raising processes obliterate fine distinctions between complexes, or between spaces.* A *regular neighbourhood* of a subcomplex K of a combinatorial n-manifold M is a subcomplex $U(K,M)$ of M which (1) is an n-dimensional manifold, (2) contracts into K. If M is suitably subdivided, the closure of the set of all simplexes meeting K is such a neighbourhood. Whitehead proved the fundamental theorem that *any two regular neighbourhoods of K in M are combinatorially equivalent.* Two of the many interesting corollaries of this theorem are:

(A) *if* $|K^n|$ *is an absolute retract rectilinearly embedded in* R^k (i.e. if $|K^n|$ is of the homotopy-type of a point), $U(K^n, R^k)$ *is a k-element if* $k \geqslant 2n+5$;

(B) (proved in paper 32) *if two 3-manifolds* M^3_1, M^3_2, (*without boundary*) *have smooth differentiable loci, and the same simple homotopy type, then* $M^3_1 \times \sigma^k \longleftrightarrow M^3_2 \times \sigma^k$ *if* $k \geqslant 5$.

Among the many applications that have been made of (A) was his proof many years later (81, 1957) that a certain n-manifold is a combinatorial n-sphere, but admits an involution whose fixed points form an $(n-1)$-manifold well known not to be (even topologically) an $(n-1)$-sphere. This was the first example of a 'nice' set realizing this possibility.

The change in Whitehead's interests from strongly geometrical to predominantly algebraic topology was not, like the change from differential geometry to topology, an abrupt one, but was brought about gradually by his search for invariants to characterize the homotopy-type of complexes, and for methods of computing their homotopy groups.

His first algebraic attack on the homotopy-equivalence problem was in the paper (34, 1941) in which purely algebraic 'L- and L^*-equivalences' between incidence matrices of complexes, with elements in the Reidemeister group-ring, were shown to be necessary conditions for the complexes to have the same simple homotopy type, and the same homotopy type, respectively. This did not prove a very easy piece of apparatus to handle, but it enabled him to construct the example referred to above, showing that two complexes may have the same homotopy type but different simple homotopy types; and, more important, to give a complete homotopy classification of the (3-dimensional) lens-spaces. A necessary and sufficient condition for the lens-spaces of types (p,q) and (p,q') to be homotopy-equivalent is (he showed) that either qq' or $-qq'$ be a quadratic residue mod p. This result has been the basis of many counter-examples constructed since that time, including Milnor's for the Hauptvermutung (1961).

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* I have not found it possible to state these theorems quite accurately in the absence of definitions too elaborate to be given in this memoir. The 'contractions' of this paragraph are nearly, but not quite, the same as those defined above. A *combinatorial n-element* and $(n-1)$*-sphere* are complexes combinatorially equivalent, respectively, to an n-simplex and its boundary. In a *combinatorial n-manifold* the closure of the simplexes containing any vertex a is the join of a to an $(n-1)$-sphere or $(n-1)$-element.

Towards the end of the war there appeared the paper (37), in which he computed various homotopy groups of the Stiefel manifold $V_{n,m}$, the space of orthogonal m-tuples at the origin in R^n. This paper suffered severely from various stresses and strains of the times. There are obscurities not only in the subject-matter, to which he was unable to give an effective final revision, but in the relation of his work to that of Hurewicz, to whom he ascribes notions not to be found in Hurewicz's published works. The long delay in publication (received 1941, appeared 1944) diminished the direct influence of the paper. Finally some of the groups were found to be wrongly computed. Nevertheless this paper was of great importance since in it there was introduced for the first time the *homotopy sequence* of a space and subspace, which he proved to be exact in the last paragraphs of the paper and from which he there derived the homotopy sequence for fibre-spaces.

The misfortunes and mistakes of this paper had a profound effect on Whitehead's later work. He was himself convinced that the rather free geometrical style of writing used in it was partly to blame for the troubles, and in the autumn of 1946, which both he and I spent in Princeton, he resolved to adopt a more algebraic form of statement in his future writings, a resolution which he kept, though he never ceased to look for geometrical meanings. It finally led him to undertake what amounted to a complete re-statement of his earlier work on homotopy, in the papers (46, 47 and 50, 1949-1950).

The basis of this revision was the notion of the *CW-complex* (46) which soon came to be regarded as the proper category of objects for homotopy theories. It was suggested to him by the process of attaching and detaching spheres which had played an important part in the proofs of theorems about 'm-groups' in the original treatment in (28).

A *CW* (i.e. 'closure-finite weak-topology') complex is a Hausdorff space presented as the union of disjoint 'cells'. The closure, $\bar{e}^n$, of an n-cell e^n is given as the image of a mapping, f, of the unit ball B^n, and the following conditions are imposed: (1) $f \mid \operatorname{int} B^n$ is a homeomorphism on to e^n; (2) $f(\dot{B}^n)$ meets only a finite number of cells, all of dimension $< n$; (3) (the 'weak topology') a subset of X is closed if and only if it meets each $\bar{e}^n$ in a closed set. A *CW*-complex may be thought of as built up by the addition of successive cells, with singular boundaries, but disjoint and non-singular interiors, the cells of lower dimension coming first. Subject to (2) and (3), the boundary of a new n-cell may be mapped in any way into the union of cells of dimension $< n$ already present. The CW-complexes, though far more flexible than polyhedra, have many of their most useful properties. They are locally contractible, and (owing to the weak topology) they have the 'homotopy extension property', that is, every homotopy defined over a subcomplex can be extended over the whole space. This property he used (45) to obtain the important and still often used theorem that two connected CW-complexes X, Y, are homotopy-equivalent if, and only if, a map f: $X \to Y$ exists which induces an isomorphism of the homotopy groups. Moreover if the dimensions are finite and $\leqslant n$, the isomorphism of the groups up to π_n suffices.

In the paper (50, 1950) the theory of simple homotopy type based on the Reidemeister invariant (34, above) was recast, with a new definition based on the *torsion* (a generalization of the Reidemeister-Franz torsion). This is a certain function, τ, of homotopy-equivalences, ϕ: $K \simeq K'$, between CW-complexes, with values in an abelian group. K and K' have now, by definition, the same simple homotopy type if there exists a ϕ with $\tau(\phi) = 0$; and this was shown to be so if and only if $K \to K'$ by transformations which are natural generalizations, for CW-complexes, of the 'elementary expansions and contractions' of simplicial complexes. Thus the meaning of 'simple homotopy type' is substantially unchanged. This version of the theory has not been found by others to be much easier to handle than the first; but a modified form of the torsion has lately been used by Milnor to prove (as part of the counter-example for the Hauptvermutung) that $L_1 \times S^{2n}$ and $L_2 \times S^{2n}$ are not homeomorphic if L_1 and L_2 are the (7,1) and (7,2) lens-spaces, and n is large enough.

In the series of substantial papers (43, 47 to 50, 52) Whitehead continued to explore the possibility of finding a full algebraic characterization of homotopy type. For n-connected complexes* of dimension $\leqslant n+3$ (the so-called A^n-complexes) he found such a full invariant (43, 49) in the *cohomology system*, a set of cohomology groups and homomorphisms; and he finally came very near to success in the general case with an exact sequence, given in its final form in (57, 1950). This is the sequence

$$(1) \ldots \to \Gamma_n \xrightarrow{i} \pi_n \xrightarrow{j} H_n \xrightarrow{\beta} \Gamma_{n-1} \to \ldots$$

where H_n is the nth singular homology-group of the space X; $\pi_n = \pi_n(X)$; and if X is a CW-complex, Γ_n is the group of equivalence-classes of maps $S^n \to X^{n-1}$ for homotopies in X^n.† Since j is the homomorphism used by Hurewicz in his fundamental 1935 paper, and Γ_n is trivial when X is $(n-1)$-connected, the exactness of the sequence generalizes Hurewicz's theorem that the first non-vanishing homotopy and homology groups are isomorphic. Whitehead found an equivalence relation between the sequences (1) of two spaces, which does not quite provide the full classification; but when later

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* I.e. $\pi_r(X) = 0$ for $r \leqslant n$.

† X^n = n-section = union of cells of dimension $\leqslant n$. For spaces other than CW-complexes the definition of Γ^n is more complicated.

Postnikov found a full system of invariants for homotopy-type, Whitehead was able to show that it could be derived from his sequence.

A problem to which he returned again and again was the computation of the homotopy groups of spheres, one of the principal objects of algebraic topologists' activity in the post-war years. His first direct contribution was the paper (33, 1941), in which he defined the multiplication which later became so generally known as the 'Whitehead product' that he was finally compelled to use the name himself (66). By modifying a geometrical argument of H. Hopf he showed that, if $S^m \vee S^n$ is the union of two spheres with a single common point, $\pi_r\ (S^m \vee S^n)$ is the direct sum

$$\pi_r(S^m)+\pi_r(S^n)+\pi_m.\ \pi_n$$

where $\pi_m.\pi_n$ is the subgroup generated by all the Whitehead products $\alpha.\beta$, $(\alpha \in \pi_m(S^m),\ \beta \in \pi_n(S^n))$, and is (as he also showed) cyclic infinite. The Whitehead product was the principal instrument used by Hilton (1955) in the subsequent calculation of the homotopy groups of a 'bunch' of spheres with a single common point. In the 'Note on suspension' (53, 1950) the problem of computing homotopy groups is attacked in a combinatorial spirit, through a generalization of the Freudenthal suspension theorems.

More distantly related to this work was the series of papers with E. H. Spanier (64, 74, 79, also Whitehead's presidential address to the London Mathematical Society, 78, 1956) in which they were seeking for a duality principle in homotopy, as were many others at that time. The results are formulated in what they called (64) an 'approximation to homotopy theory', or S-theory.

The *suspension*, SX, of a space X is its join to two points, a process which may evidently be iterated to give a space S^rX.

A map $f:X \rightarrow Y$ determines in an obvious way a map Sf: $SX \rightarrow SY$. By a natural generalization of the notion of homotopy, maps $g:S^pX \rightarrow S^pY$ and $f:S^qX \rightarrow S^qY$ are 'S-homotopic' if $S^{r-p}g \simeq S^{r-q}h$ in S^rY for some $r \geqslant$ max. (p,q). By using this equivalence as a classification, an abelian group $\{X,\ Y\}$ of 'S-mappings' was defined. If P is a polyhedron embedded in S^n, a polyhedron Q in S^n-P is an *n-dual* of P if it is a deformation retract of S^n-P. The name is justified since the relation is symmetrical. It was shown by Spanier and Whitehead that if (P_1,Q_1) and (P_2,Q_2) are dual pairs, the groups $\{P_1,\ P_2\}$ and $\{Q_2,\ Q_1\}$ are isomorphic. The theory has been applied to the problem of embedding manifolds in spheres.

In the last three years of Whitehead's life there was a great revival of geometrical topology of the kind that he had worked on twenty years before. It was due in the first place to the appearance of a proof by Papakyriakopoulos (1957) of the long outstanding Dehn lemma, and secondly to the 'collar' theorems of Mazur (1958) and Morton Brown (1960) whose methods led on eventually to the proof of the Poincaré hypothesis for $n \geqslant 5$ (Smale, Stallings, Zeeman). The Dehn lemma is a specifically 3-dimensional theorem: *if a* *semi-linear map of a disk into an orientable M^3 has no singularity in the boundary polygon, C, its singularities can be entirely removed,* i.e. C bounds a non-singular 2-element in M^3. Dehn published a faulty proof in 1912; Papakyriakopoulos finally proved the lemma in 1957, and, at the same time, his own related 'sphere theorem'. This says that in an M^3 of a certain general kind, the existence of a *singular* essential 2-sphere implies the existence of a *non-singular* essential 2-sphere.

The Papakyriakopoulos theorems aroused Whitehead's deep interest. He and A. Shapiro soon produced (84) a greatly simplified version of the proof of the Dehn lemma, and Whitehead himself (83) managed to free the sphere theorem from Papakyriakopoulos's rather troublesome restriction on M^3. One of Whitehead's last works (88), a proof that every connected 3-manifold, M^3, which is not compact can be *immersed* in R^3, also derived from his work on the Dehn lemma. (A map $f\colon\ M^3 \to R^3$ is an immersion if each point of M^3 has a neighbourhood, U, such that $f|U$ is an embedding.)

But it is the still more recent geometrical theories initiated by the work of Mazur and Morton Brown, that have revived the spirit of the early combinatorial theories, with applications of many of the results of Whitehead's great 1939 paper. His last collaborative paper (90) with R. Penrose and E. C. Zeeman, deals with the problem of embedding a compact n-dimensional manifold in a Euclidean r-space. How small can r be? That R^{2n} is big enough to hold any smooth combinatorial M^n has been known since 1944 (Whitney). It is shown in (90) that the vanishing of the successive homotopy groups allows r to be lowered *pari passu : every closed k-connected* n-manifold can be embedded in* R^{2n-k} *if* $0 \leqslant k \leqslant \frac{1}{2}n-1$.

Everything depends on the lemma that *if an embedding, h, of a polyhedron* P^{m-1}, ($m \leqslant \frac{1}{2}n$) *as a subcomplex of the interior of an n-manifold is homotopic to a point, there is an n-element, E, such that* $P \subseteq \text{int } E \subseteq E \subseteq \text{int } M$. The main steps in the proof are as follows. The existence of the homotopy to a point implies that h can be extended to a mapping of a cone vP^{m-1} into int M. It is first shown that h can be extended to an *embedding* h, of vP^{m-1} into int M. (For cells of dimension less than m this follows easily from $m \leqslant \frac{1}{2}n$, for cells of the top dimension less easily.) Now the 'cone' $h(vP^{m-1})$ is collapsible to its vertex $h(v)$. Therefore by Whitehead's theorem (A) above, any regular neighbourhood of $h(vP^{m-1})$ in M is the required n-element. This lemma played an essential part in Stallings's original proof of the Poincaré hypothesis for $n \geqslant 7$.

Simultaneously with his renewed activity in strictly combinatorial topology, Whitehead was working in 1959 on the problems of *transverse k-fields* of an n-manifold, M^n, embedded in R^{n+k} (89). A mapping, ϕ, of M^n into the Grassmann manifold of k-planes through the origin in R^{n+k} defines a transverse k-field if for no three points x, y, z of M sufficiently near to each other is the line xy parallel to the k-plane $\phi(z)$. Whitehead considered particularly whether the existence of a transverse field implies, or is implied by, the existence of a differentiable structure compatible with a given triangulation of M. He showed that the direct implication (existence of field implies differentiable structure) holds if M^n is a combinatorial manifold rectilinearly embedded. The more difficult reversed implication he considered only for $n \leqslant 4$, and showed that it is true only of certain embeddings. From such theorems the existence of differentiable structures can be inferred for rectilinearly embedded combinatorial manifolds. In this paper too, the methods are predominantly geometrical, and even analytical.

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* See footnote p. 357.

The three papers (88, 89, 90) all written within a few months of his death, and published after it, give a vivid impression of the intensity and variety of his mathematical life up to the moment when it was suddenly broken off.

I have been helped in the preparation of this memoir by many friends of Henry Whitehead. I am particularly grateful to Dr M. G. Barratt for help with the section on scientific work, to Professor Hilton for allowing me to see his own memoir (to appear in *Enseignement Mathématique*), to Dr Ioan James for allowing the use of the bibliography prepared by him, and to Mrs Whitehead for many conversations and consultations.

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